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A DIMENSIONLESS PHYSICS? *

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DURING the last twenty years a number of fundamental physical problems have emerged which require new methods. Hence the proposals for using non-linear equations, S-matrices, non-localisable fields, fundamental lengths, and interaction representations. The present paper is based on the more radical assumption that *atomic theory has now reached a point where dimensional variables are inappropriate and only dimensionless methods can provide insight into basic problems.* Here we are not concerned with the truth or falsity of this assumption, which cannot be ascertained today, but with its value as an instrument for developing new methods of attacking old problems.

By ' dimensionless ' is meant ' without dimensions in the units of length, time, or mass '. Angles may be measured in degrees, radians, or right angles, but are dimensionless in this sense, as Fourier observed in 1822. ' A dimensionless method ' is one in which all the variables and particularly those of length, time, and mass, are expressed in dimensionless form. The present analysis is intended to clear the ground for mathematical treatments of particular problems.

It is often assumed that though dimensional arguments [1] may assist the selection of empirical rules they are of little value in the solution of fundamental theoretical problems. Indeed the theoretical physicist tends to regard the dimensional aspects of atomic problems as trivial. But Bohr was led to his theory of atomic structure by the observation that h had the same dimensions as angular momentum, and Einstein's suggestion, made in 1909, that since e^2/c and h have the same dimensions they should be traceable to a common source in spite of much work has not yet led to a satisfactory result.

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* Based on the Chairman's Address to the Philosophy of Science Group, 12th October 1953. See also the cognate paper : ' Dimensionless Secondary Quantities ', *Amer. J. Phys.*, 1953, **21**, 323 (referred to here as ' D.S.Q.').

[1] The following are recommended on traditional Dimensional Theory : P. W. Bridgman, *Dimensional Analysis*, New Haven, 1931, and article on D.A. in *Encyclopedia Britannica*, 1947 and later. H. L. Langhaar, *Dimensional Analysis and Theory of Models*, New York, 1951. A. E. Ruark, *J. Elisha Mitchell Sci. Soc.*, 1935, **51**, 127. Some basic points are particularly clearly stated by H. Jeffreys, *Phil. Mag.*, 1941, **32**, 177 (see pp. 180–183).

The neglect of dimensional theory by many of the leaders of physics during recent decades—Bridgman is one exception—has had unfortunate consequences. Buckingham's Π Theorem,[1] which is basic in this field, was published in 1914, and Hersey's discussion[2] of the relation of partial differential equations to Buckingham's Theorem appeared in 1916. Nearly forty years have passed, yet no adequate discussion of this work has appeared illustrating the Theorem with examples not only from the fundamental branches of classical, relativity, and quantum physics, but also from continuum mechanics, hydrodynamics, thermodynamics, and the theory of deformations. That would make a useful subject for a Thesis.

The empirical mind may be tempted to regard the methods of Buckingham and Hersey as too abstract and generalised, though dynamics only becomes mature through a similar generalisation. But on the empirical side the neglect is as great and more surprising. For, unless I have been wrongly advised and my own searches inadequate, *no comprehensive survey or list has been made of all the dimensionless quantities, groups and parameters, variables and constants, of current physics.* There is another theme for a Thesis.

For present purposes Buckingham's Theorem, shorn of its all-important mathematical expression, reduces to the assertion that every complete[3] physical equation can be expressed in dimensionless form. We propose the further step of assuming that only if it is so expressed can outstanding problems be solved. This is not trivial, because it forces one to discover the appropriate dimensionless expression.

Our assumption is that *the canonical expression of the laws in the future*

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[1] E. Buckingham, *Phys. Rev.*, 1914, **4**, 345 ; *J. Wash. Acad. Sci.*, 1914, **4**, 347

[2] M. D. Hersey, *Scientific Papers of the Bureau of Standards*, 1919, no. 331

[3] For Buckingham 'complete' meant : in which no relevant quantity is overlooked.

atomic theory is dimensionless in the units of length, time, and mass.[1] It follows that the canonical form of any quantitative assertion, or of any complete set of dimensional constants, is also dimensionless.

One implication is that an isolated assertion such as ' light in vacuo displays a velocity c ' is forbidden. Dimensional constants are only significant when combined into appropriate dimensionless ratios, though not all such ratios are significant, as we shall see. To broaden our understanding we have to learn to think dimensionlessly, and more than that, to use only the right combinations.

A further implication is that the Maxwell and Schrödinger-Dirac field equations must be transformed into dimensionless variables and combined into one expression so that only dimensionless constants are present, if their basic significance is to be discovered. Thus the relativity principle (c) and the quantum principle for electrons (e^2, h) must be re-interpreted as the dimensional consequences, possibly only valid under special conditions, of a more general dimensionless principle involving α, the general interaction parameter for electrons-photons, still somewhat misleadingly called the fine-structure constant.

The meaning of the assumption that the canonical atomic laws are dimensionless may be clarified by a simple geometrical illustration. It may be interpreted as implying that e^2/mc^2 and h/mc (where $m =$ electron rest-mass), being lengths, may ' correspond ' to two sides of a triangle whose angles are functions of α, and that it is α that matters for basic theory. But at this stage we do not specify the meaning of that ' correspondence '. It is enough that the basic theory, being dimensionless, may use angles but knows nothing of single lengths. To speak of the ' length of a side ' is misleading, for on the present assumption lengths only possess fundamental meaning when coupled as a dimensionless ratio. Linear metric and dimensional quantities enjoy restricted validity and only become fully reliable when combined to form dimensionless groups.

It is certainly not too early to prepare the ground for a systematic dimensionless theory, for what is probably the first valid theorem in dimensionless atomic physics was formulated 55 years ago before h was introduced. Larmor, in his *Aether and Matter* written in 1898, suggested that ' the orbital velocities of the electrons are about of the same order of smallness (exceeding 10^{-3}) compared with the velocity of radiation as are the molecular dimensions compared with the wave-lengths '.[1] In fact both ratios are of the order α, if the quantum numbers are small. This anticipation deserves to be remembered, and significantly it equates a ratio of two natural velocities to a ratio of two natural lengths, a form which must play an important role in any dimensionless theory.

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[1] Cf. ' Atomic principles may find their most powerful expression in angles '. This *Journal*, 1952-53, 3, 256

As irreversible processes are now attracting attention it may be useful to give an example from that realm. In 1937 Zwicky suggested that : ' scientifically speaking *history means the change in time of dimensionless ratios of significant physical quantities* ',[2] an idea implicit in many subsequent cosmological speculations, such as those of Dirac. More will surely be heard of this, for cosmology—like other branches of physics—can speak most clearly in dimensionless language, as we shall see in a moment.

Actually it is very late in the day to call for greater attention by fundamental theory to dimensionless quantities, for they have been appearing in great numbers in most realms of physics since the end of the last century. From 1600 to 1880 the only dimensionless quantities in regular use (with a few exceptions such as Poisson's ratio and the ratio of the specific heats) were *numbers of entities*, *angles*, and *harmonic phases* in wave motions. These were matters of counting or of simple geometry or kinematics, and their dimensionless character raised no theoretical problems. But since the classical indifference to particular numerical values was replaced around 1900 by the relativistic and quantum emphasis on special values (limit velocities, fundamental charges and masses, *eigenwerte*) and increased attention began to be paid to the structure of more complex interactions, dimensionless numbers have been emerging at nearly all the growing points of physics. Each such number is a signal of growth claiming our attention and undoubtedly bearing a significant message, though in a language we cannot yet read. For example, each may be announcing the presence of a *structural angle* in the laws determining the systems, but that we cannot yet say. Whatever their meaning it would be foolish to neglect them. Here are the most important. The following list is not a systematic classification, for the types overlap. The dates are approximate.

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[1] J. Larmor, *Aether and Matter*, Cambridge, 1900, p. 233

[2] F. Zwicky, *Proc. Nat. Acad. Sci.*, 1937, **23**, 106

Dimensionless numbers

Universal constants	e^2/hc (1909). M/m (1916), followed by other mass ratios. GMM/e^2 [1] (1929). Ratios of lives of unstable atoms (1920 . . .). Cosmological numbers (1929).
Material constants	Poisson's ratio, ratio of specific heats, etc. (nineteenth century).
System constants	The Stokes-Reynold's Number (1850), followed, mainly in this century, by some *twenty-five* other dimensionless groups, representing gravitational, mechanical, molecular, thermal, and electrical properties.
Non-integral exponents in empirical expressions (either material or system constants)	1/3 power of time in the Andrade expression for creep (1910), and thereafter fractional or variable exponents for semi-conductors, partially turbulent flow, parachor, mixed type heat engines, etc.
Quality factors, coupling parameters	Parameters measuring sharpness of resonance, degree of interaction causing perturbation from periodicity, or change of frequency per period. Mainly after 1930.
Variables expressed in natural units (l_0, t_0, v_0, etc.)	l/l_0, t/t_0, v/v_0, etc. Quantum Numbers (1913), later generalised to charge, mass, etc. Velocity factor (v/c) in Fitzgerald - Lorentz - Einstein relativistic expressions (1882 . . .) l_0/r in Yukawa's potential. Dimensionless co-ordinates, e.g. in crystal theory.

This is an impressive list and it suggests some questions. What is common to all these numbers ? How are the universal, material, and system constants related ? What laws do these constants and variables obey, what principles do they illustrate, and what mathematical expressions best display their properties ? Which possess sign, sense, or orientation? How are they related to reversibility and irreversibility? Which, like α, represent ratios involved in some type of interaction? Which can be identified as angles?

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[1] $G =$ Gravitational constant

These are constructive questions opening the way towards a systematic theory of dimensionless physics. What would be more appropriate than that dimensionless groups should develop from an obscure dodge for simplifying empirical data into the supreme calculus for elegant and comprehensive formulation?

This may be good sense, but it is certainly not yet common sense. For two generations physics has been marked by the emergence of dimensionless numbers of increasing importance, yet no general examination of their meaning has been undertaken. It is a hundred years since Stokes in 1850 defined the Stokes-Reynolds Number, and forty-four since Einstein in 1909 noted the challenge presented by the ratio e^2/hc. Approximate derivations of the critical value of the Stokes-Reynolds Number have been published recently, but *there is no acceptable theory of any of the dimensionless constants of atomic physics.*[1] The first half of the century was a period of the accumulation of theoretically arbitrary dimensionless constants; the second should achieve their derivation as an integral part of a dimensionless theory of particles. It has been evident to many since 1928-29 that—to use Einstein's words[2]—' in a reasonable theory there are no dimensionless numbers whose values are only empirically determinable' (i.e. not theoretically derivable), and that the appeals to quantitative experience must either be eliminated (Eddington, Milne), or reduced to a minimum.

A classical mind, whose formative years had been occupied solely with problems characteristic of the pre-quantum period, might object: ' Physics is concerned with general laws; too much attention should not be paid to special numbers '. There would be force in this, if dimensional physics were satisfactory and if nature were not bombarding the physicist with special numbers at an increasing rate. But dimensional physics is in several respects unsatisfactory. For no lucid major advance has been made since 1929, and the unsolved problems of nuclear, meson, and relativistic-quantum theory are notorious. What is a rigid rod in relativity theory, and what is a satisfactory clock in quantum theory? Since these basic questions have not been answered, special difficulties should not surprise us. Moreover, in the context of a quantum age dimensional expressions are beginning to look intrinsically unsatisfactory, as though they were outmoded instruments that should not be patched up but replaced by a more efficient design suited to the needs of the time.

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[1] Eddington's work on the constants is unacceptable to most competent judges. His theory of the fine-structure constant must be rejected because it throws no light on electron-photon interactions, apart from questions of obscurity, reliance on unadmitted empirical data, uncertainty regarding the value, etc.

[2] Albert Einstein, *Philosopher-Scientist*, ed. Schilpp, New York, 1949-51 (Letter of 13th October 1945), p. 144

This suggestion may appear dubious, but I believe that it only makes explicit what Eddington, Milne, and others have felt at different times from 1928 onwards: if it were possible, a dimensionless physics would be more satisfactory. And this means that dimensional physics has already been realised to be in some respect essentially *un*satisfying.

Dimensional laws offend our desire for completeness and elegance. They are in certain respects incomplete, misleading, and over-complex.

They are *incomplete*, because they do not show how the variables are to be measured, the theory of measurement being treated independently. This would not matter if measurement were a general procedure unrelated to particular laws. But it is not. The methods used to determine electron momentum are entirely different from those necessary to measure a transit time and a velocity. The conception of one general quantum theory of measurement is mistaken and to the detriment of theory tends to conceal the variety of the different procedures which are actually used. Only a dimensionless formulation can force the physicist to make explicit the standards of comparison employed in measuring any dimensional variable.

Take an example from cosmology. The assertion: 'the universe is expanding' is incomplete; no standard is specified. Cosmologists do not appear to have agreed whether this means, for example, relative to a rigid rod, an optical wave-length, an electronic length, or the cube root of the volume of a standard extragalactic nebula. The choice should be determined by the character of the observations that have been made, and is important because the theoretical interpretation to be given to the phenomenon will be different in each of the four cases. Only a dimensionless formulation[1] can eliminate this ambiguity, and only a dimensionless calculus can ensure correct treatment, just as an invariant calculus (in relation to co-ordinate transformations) was necessary to ensure conformity to the principle of relativity. After ordinary tensor and wave tensor theory the natural next step is to dimensionless theory.

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[1] See Zwicky, loc. cit. G. J. Whitrow, *Phil. Mag.*, 1946, 37, 469. *R.A.S. Monthly Notices*, 1951, **111**, 455

Next, dimensional laws are *misleading*, and may lead to incorrect conclusions, because they implicitly claim accuracy over all ranges of the variables, whereas in every realm the form of the appropriate law and its degree of accuracy depend on the order of magnitude of the quantities involved, compared with some appropriate natural standard. This pretence of unrestricted validity and accuracy was appropriate to classical expressions, but should be banished from contemporary physics, except for the idealised classical limiting cases.

Since 1916 [1] it has been clear that the Coulomb law could not be expected to be valid at distances comparable with e^2/mc^2, and this has been confirmed experimentally. This fact should be expressed by stating the law in a dimensionless form which explicitly limits its accuracy over some definite range, or by transforming the law so as to produce an equivalent result. A hint as to how this might be done is implicit in the example given later.

The root of the trouble is that dimensional laws fail to render explicit the fact [2] that current atomic theory, being based on classical analogies, is merely an approximative method which works *only because* $\alpha \ll 1$ (and electron/proton mass $\ll 1$). This should be made clear by using a dimensionless calculus which yields results *only* in the form of power series in α, as in current Quantum electro-dynamics. That would emphasise unmistakably the fact that the established dimensional laws must be viewed as asymptotically valid Limit Laws of some unknown General Law.

Finally laws involving dimensional constants *contain a redundant element*, for, as Whitehead observed,[3] the presence of a dimensional constant implies that ‘ a possible definition of congruence has been omitted ’. Thus the presence of c means that the space-units and the time-units are connected, and the presence of e^2/h that they are also connected in a second different manner. The multiplicity of dimensional constants arises partly because physical theory has neglected certain latent congruences or ways of deriving time-measures from space-measures (or vice-versa). When so many dimensional constants are available to provide all that is needed, it produces unnecessary complexity to treat t as an *independent* variable. For that is precisely what it is not.

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[1] e.g. see O. W. Richardson, *Electron Theory of Matter*, Cambridge, 1916, p. 555

[2] N. Bohr, *J. Chem. Soc.*, 1932, **1**, 349. It is not surprising that experimental physicists often fail to present a properly formulated estimate of uncertainty (standard error) in publishing their results, if physical laws are formulated so that they conceal a known systematic bias, as is now the case.

[3] A. N. Whitehead, *Principles of Natural Knowledge*, Cambridge, 1919, p. 164

This redundancy, which was also recognised by van Dantzig,[1] may be viewed from another side. It is clumsy to postulate a continuous scale-free metrical frame and then to impose on it scale-fixing restrictive conditions determined by e^2/mc^2, $h^2/4\pi^2me^2$, etc. For these natural lengths are involved in the physical measures which are employed in establishing co-ordinates. There is no need to use both a linear metric and dimensional constants ; it would be simpler to express the constants in a different manner so as to provide a theory showing how metrical co-ordinates can be established. And to avoid assuming a linear metric this method must be dimensionless. This suggests a line of escape from the present dilemma of using dimensional co-ordinates largely in order to discover that they break down (indeterminacy, non-localisation, S-matrix, etc.).

These blemishes in dimensional laws are interconnected, and can be regarded as the consequence of continuing to employ too narrow a conception of physical law in relation to the procedures of measurement. To make this clear we shall contrast two alternative types of physical theory, A and B, neglecting the fact that transitional forms exist. A is well-known and its value unquestionable, though perhaps nearly exhausted. B is relatively untried, though a number of interesting intellectual experiments of this kind have already been made.

(A) Theories based on a linear metric and external linear measures. The universe is divided into two parts : systems (S) to be measured, and arbitrary measures (M) of length and time, external to S, which are not analysed in terms of any more general conception. The numbers obtained by measurement vary with the units of L, T, etc. The laws cover only relations between these numbers, the representation of the interactions (I) involved in the various procedures of measurement lying outside their scope. The theory obtained is dimensional, the system is conceived in a linear-metrical co-ordinate frame, and measurement involves the determination, as closely as possible, of coincidences of point-events within that frame.

Symbolically : the universe $U = \Sigma S \leftarrow I \rightarrow M$, the laws representing

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[1] D. van Dantzig, *Proc. Roy. Akad. Amst.*, 1934, **37**, 521 ; 1940, **43**, 387, 609 ; and elsewhere

S and M, but not I. The redundancy springs from the fact that certain systems (rods, clocks) fall under both S and M.

The whole of physical theory from Galileo to Dirac forms part of the progressive development of this method. The special position of quantum mechanics will be discussed under B.

(B) Theories based on some geometrical property more general than or independent of a linear metric (such as distant parallelism, conformal invariants, angles), and providing a theory of measurement showing under what conditions and with what accuracy linear co-ordinates (x, y, z, t) can be established. Since no external linear measures are to be assumed, the basic expressions must be dimensionless. The internal spatial structure of any system (S)—e.g. the system of angles of a static point lattice or molecule, or of a changing angular pattern—is represented directly in terms of angles, which do not require reference to arbitrary external measures. The various procedures of linear measurement, the measures used, the interactions employed, and the quantitative relations obtained are all treated as a part of S, or of the processes of S. The system may be conceived as defined by angular relations (numbers of particles and angles formed by their patterns) and all processes of measurement as based ultimately on the determination of angular equalities and inequalities and the stability or instability of patterns.

Symbolically : $U = \Sigma S$, the laws representing the processes of S (one-way processes), including the interactions (I) necessary in linear measurements.

No fully developed examples of B are yet available but the following work lies in this direction :

(*a*) Quantum mechanics displays some features of B, such as the use of dimensionless numbers, and the Bohr-Born-Heisenberg-von Neumann theory of measurement has sought to bring the procedures of measurement within the quantum mechanical calculus. Moreover, the procedure of *normalisation* implies that in certain problems it is the direction not the length of a vector that is important. But (i) linear co-ordinates and external measures are assumed in all treatments ; (ii) Hilbert space possesses a quantitative property analogous to linear extension and the Dirac symbolism is linked with a linear metric through the quantum conditions ; and (iii) von Neumann's theory[1] is incomplete and has failed to analyse particular procedures, e.g. those required to justify a transit time. Bohr's suggestion that the measuring apparatus should be regarded as part of the system observed corresponds to B, but cannot be carried out without a complete theory of particle interactions.

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[1] J. von Neumann, *Mathematische Grundlagen der Quantenmechanik*, Berlin, 1923. See E. Schrödinger, Essay on de Broglie, *Festschrift*, Paris, 1952

(*b*) Born, Dirac, and Milne have at times sought to use dimensionless expressions in fundamental theory, but without unquestionably constructive results.

(*c*) Küssner [1] based an obscure speculative theory on numerical methods applied to describe point-events, without using a linear metric.

(*d*) Van Dantzig [2] and Ingraham,[3] following Kottler and many others,[4] have developed theories of realms of continuum physics without a linear metric in the fundamental expressions. Van Dantzig uses conformal invariants (angles) and tensor densities, in place of the linear interval and the basic tensor, to develop a complete theory of electromagnetism independent of metric. Ingraham also drops the invariant 4-D interval, retaining only angles, and thus obtains a theory of Conformal Relativity leading towards a new type of unified field theory. Ingraham's Conformal Relativity may crown the non-quantised field theories, displaying all their logical and mathematical inter-relationships. But neither of these authors has developed a theory of measurement showing how linear co-ordinates are established.

(*e*) Eddington's work on dimensionless numbers is of greater interest than any other investigations in this field, not because it achieved any clear success, but since his concern with the problems of physical theory, epistemology, and measurement theory led him further than anyone else. He made mistakes, but where no-one had been before him. Here we are concerned only to point out where the methods now proposed diverge from his.

The Wave Tensor Calculus is necessary because the ordinary tensor calculus cannot conveniently deal with *angular* changes. In certain passages Eddington appeared to be guided by the aim of a dimensionless theory,[5] in the sense used here. 'The quantitative part of the specification must therefore consist of undimensional quantities.'[1] Yet in his *Fundamental Theory* he used arbitrary external standards of lengths and dimensional quantities and even denied any alternative possibility :

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[1] H. G. Küssner, *Principia Physica*, Gottingen, 1946. Review in *Nature*, 1948, **162, 44**

[2] loc. cit.

[3] R. L. Ingraham, *Proc. Nat. Acad. Sci.*, 1952, **38**, 921 ; *Nuovo Cimento*, 1952, **9**, 886

[4] See E. T. Whittaker, *History of the Theories of Aether and Electricity*, **2**, 192–196, for detailed references

[5] On Eddington's carelessness with units see H. Jeffreys, loc. cit., pp. 183–184

> The internal structure of a system can be described wholly by numerical ratios ; but to complete the description it is necessary to fix the scale of the system by reference to some standard outside it. If the whole universe were being investigated as one system, an outside standard would be unnecessary. But the analytical method of physics divides the universe into simple systems of various types which are studied one by one. Each system is supposed to be surveyed from outside, so that the observer and his standard scale are alien to the system. The extraneous standard forms the link that enables us to put together in a definite scale-ratio the fragments into which our analytical method divides the universe.[2]

But any theory which studies simple systems one by one necessarily neglects their interactions. Moreover, to complete the description of a system S_1, it is *not* ' necessary to fix the scale of the system by reference to some standard outside it ' in Eddington's sense of fixing the scale by reference to an external *linear* standard. For if S_1 is treated as part of a wider system S the quantitative relation now regarded as ' the linear scale of S_1 relative to an external linear measure M ' can be represented equally well, and perhaps better, as an angular relation of the angular pattern of S_1 within the whole angular pattern of S.

Eddington's desire to move towards a dimensionless theory was frustrated by his failure to observe that a genuinely dimensionless theory, incorporating the measures and the interactions within the system S, requires that linear metric must be eliminated from the postulates. Thus he was left in the confused position of seeking to maintain two propositions simultaneously : *quantum theory is a pure number theory* and *a linear metric is implicit in its physical interpretation*, for example of the quantum conditions. The second is correct, but the first is a misleading half-truth.

(*f*) Many workers [3] have considered the relation of quaternions to special relativity and to relativistic quantum theory. If a quaternion is defined, following Hamilton's first method, as a dimensionless quotient of two vectors (lines possessing length, orientation, and sense), the introduction of quaternions may be regarded as a step towards a dimensionless theory. We can interpret Tait's cry, ' Repent Cartesian sins and embrace the true faith of quaternions ! ' as meaning ' Drop lengths and substitute angles ! ' Kilmister [1] has shown that Eddington's formulation of Dirac's equations can be simplified by using quaternions, and interpreted as representing the non-metrical properties of an affine space of distant parallelism. Thus Dirac's equations in Kilmister's derivation are independent of metric.

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[1] A. S. Eddington, *The Philosophy of Physical Science*, Cambridge, 1939, p. 75. See also *Fundamental Theory*, Cambridge, 1946, p. 7

[2] A. S. Eddington, *Fundamental Theory*, p. 14

[3] For references see C. W. Kilmister, *Proc. Roy. Soc.*, A, 1949, **199**, 517

(*g*) This paper forms part of a study [2] of the methods appropriate to a dimensionless atomic theory based on finite numbers of particles (? permanent nucleons) and the changing angles of their patterns in 3-space. The aim is a goniometry of the physical discontinuum and its changes, an angular calculus of changing point arrangements.

Previous discussions of dimensionless methods and employment of dimensionless expressions in atomic physics have rested on the assumption that such expressions were merely algebraically simpler than the corresponding dimensional expressions, to which they were otherwise equivalent. The distinctive feature in the present approach is the recognition that *certain types of dimensionless expression are more powerful than any dimensional expression, having no single equivalent dimensional expression.*

This fact has been discussed elsewhere.[3] Here we need only state the conclusions reached : First, dimensionless variables of the form $\pi' = l_0/l$ or t_0/t, where the suffix indicates a simple universal constant of length or time, are mathematically more general and physically more widely applicable than are dimensionless variables of the form $\pi = d_0/l^{\alpha}.\, t^{\beta}.$, where d_0 is a compound dimensional constant involving both length and time. The latter provide simple representations only where scale-free similarity properties are present, while the former can achieve this for scale-fixed properties in addition.[4] Secondly dimensional laws involving compound dimensional constants may be represented as special limiting cases of more general laws expressed in dimensionless variables of the form π'. Thirdly, since the aim is a general Law which can reduce asymptotically to existing expressions as its Limit Laws, it is reasonable to assume that this unknown law should be expressed in variables of the form π'.

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[1] loc. cit.

[2] L. L. Whyte. This *Journal*, 1952, **3**, 256 ; *Amer. Math. Monthly*, 1952, **59**, 609 ; ' D.S.Q.'

[3] ' D.S.Q.'

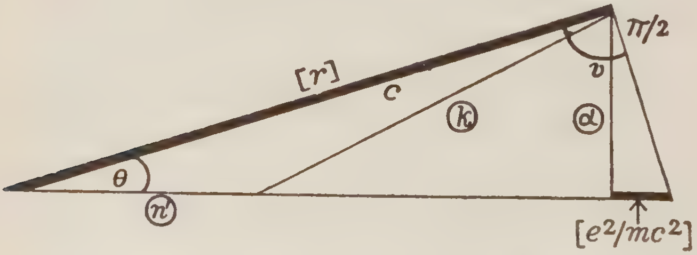
[4] Knowing the dimensions (rules for changing units) does *not* mean knowing the laws (rules governing change of size of system), since the laws may contain functions of dimensionless factors of the form l_0/l. This fact is often neglected.

These considerations are illustrated in the following example. As it will be treated in detail elsewhere, we shall here pass straight from the formulation of a problem to its solution.

Problem. Consider a system containing two types of process, involving e^2/m and c respectively. The first may be non-relativistic circular electron orbits, and the second photons with the velocity c. The problem has two parts : (i) to represent the laws of these two processes as Limit Laws of a dimensionless General Law representing phenomena involving both e^2/m and c, such as relativistic effects and/or radiation interactions ; and (ii) to quantise the system by introducing α. The problem is to be solved by providing a *simple geometrical diagram* representing the required relationships phenomenologically.

In passing we may note that the idea of an unknown General Law of Particles of which established particle expressions are Limit Laws implies that instead of treating idealised isolated particles as primary and the more general phenomena as secondary ' interactions ', the process represented by the General Law becomes primary and the separate particles are regarded as special limiting components or aspects of this primary process. The problem is to discover how certain differential equations and constants appear in particular limiting cases of some unknown General Law. In our example this means finding a geometrical General Law which reduces to two Limit Laws containing e^2/m and c.

Solution



Though in this diagram only ratios of lengths are considered significant for basic theory, individual lengths represent the dynamical parameters of the problem, as shown. $r =$ radius of orbit. n', k radial, orbital quantum numbers. $\theta =$ new dimensionless variable, shown as an angle.

This diagram[1] solves the problem, and represents all the kinematical, dynamical, and energetic parameters of the system, their ratios corresponding to ratios of lengths in the diagram. The General Law : $\{\sin\theta = v/c\,;\quad \sin\theta\,.\,\tan\theta = (e^2/mc^2)/r\}$ has two Limit Laws: I as $\theta \rightarrow 0$, $v^2.r = e^2/m$ (c dropping out) ; and II as $\theta \rightarrow \pi/2$, $v = c$ (e^2/mc^2 dropping out). In the domain between, eliminating θ from the General Law, we have the relativistic law $v.^2r = (e^2/m)\,.\,(1 - v^2/c^2)^{\frac{1}{2}}$. Moreover, when θ is such as to make n' and k integers with α as shown we have the Dirac relativistic energy levels (E) of the hydrogen atom with fixed nucleus (neglecting the Lamb corrections) given by $E/mc^2 = \cos\theta = \left\{+1\ \frac{\alpha^2}{(n' + \sqrt{k^2 - \alpha^2})^2}\right\}$. The diagram covers (i) two classical limit phenomena ; (ii) the dimensionless equivalent of the relativistic Energy expression : $H^2 = (p\,.\,c)^2 + (mc^2)^2$; and (iii) the quantised effects combining relativistic variation of mass with spin-orbit coupling. It expresses these relations as dimensionless ratios identified as trigonometrical functions of a new dimensionless variable θ, which is without physical interpretation at this stage.

It will be noted that the General Law represented in this diagram could not have been found if r had been coupled with h/mc, e^2/Mc^2, $h^2/4\pi^2me^2$, GM/c^2, or any natural length other than e^2/mc^2. We leave it to the reader to identify in this diagram other points made in this paper on the relation of dimensionless to dimensional expressions.

While the variables π', ($(e^2/mc^2)/r$, (v/c), etc.) must for the present be regarded as phenomenological parameters whose significance is unknown, it is evident that a preliminary interpretation of the π' as trigonometrical functions of angles may possess advantages. It is therefore of interest to consider the status of angles in natural systems, in current theory, and in a future dimensionless theory.[2]

Though angles and angle variables have played a considerable rôle in special applications of physical laws, the rôle of angles in natural systems appears to be greater than their absence from the fundamental laws would lead one to expect. Not only is angular motion prevalent, but constant angles determine the stable structure of matter (atoms, molecules, crystals, etc.), linear relations playing a relatively trivial part. Moreover, it is possible that angles may be even more widely present in physical systems than has yet been recognised. For small structural angles (angular deformations of molecular and crystalline structures in electric, magnetic, and other fields ; observable angular deformations of structures in linear motion relative to the observer, ' owing to the Fitzgerald-Lorentz contraction ' ; angular deformations due to rotation ; etc.) may have been neglected, empirically because very small, and theoretically either because held to be trivial or on account of their non-dimensional character. The long emphasis on linear measurements and dimensional theories may have distorted our conception of physical structure, for these methods are ill-adapted to bring out angular relations.

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[1] The simplest analytical expression of the vector relations of a plane Euclidean diagram of this kind is in terms of (complanar) quaternions, matrices, or operators similar to those of Dirac and Eddington. The Dirac Wave Equation bears a direct relation to this diagram, as regards both its general form and the values of the *eigenwerte*.

[2] See L. L. Whyte, this *Journal*, 1952, 3, 256

Another argument leads in the same direction. Recent developments in fundamental theory have been classified into those which do and those which do not introduce a fundamental length into the basic equations. But there is a third option : the elimination of linear metric. This has the advantage that it leads automatically to the appearance of one fundamental length in any linear laws which may be derivable from a dimensionless foundation. For any dimensionless theory which includes a postulate of the equivalence (indistinguishability) of all nucleons implies that any dimensional theory derived from it must contain one basic universal length.[1] Any two angularly similar systems, represented by the same set of dimensionless parameters, must be indistinguishable, and this implies that any two similar stable patterns of nucleons must in a dimensional representation be of the same scale. This in turn means that a universal length associated with all nucleons must be introduced in the transformation from a dimensionless to a dimensional representation. Thus the presence of *one* universal length in any dimensional deductions is a necessary property of any dimensionless theory employing the above postulate, and to this extent dimensionless formulations may provide a substitute for non-linear equations. It is the presence of *multiple* natural lengths, with ratios which are functions of α etc., which constitutes the real problem.

The following principles for research in fundamental theory arise from this survey :

1. All differential laws, boundary conditions, and particular solutions should be expressed in dimensionless variables of the form π'.

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[1] See L. L. Whyte, this *Journal*, 1952, 3, 256

2. Known dimensional laws are to be represented as Limit Laws of an unknown dimensionless General Law, and the first step is to convert them into dimensionless form.

3. Only the appropriate combination of dimensional constants must be used in converting a dimensional expression.

4. It may be convenient to interpret the π' as trigonometrical functions of angles which may be physically identified later.

5. Structural angles may play a greater role in physical processes than has been realised.

To these we may add another which leads beyond the points treated in this paper:

6. The future atomic theory may be based on a calculus of the one-way transformations of angular point arrangements in 3-space (? patterns of permanent nucleons), the cyclic and reversible processes of dimensional theory being represented as limiting or extremal components of these one-way transformations. It will be shown in a later paper that the class of physical phenomena which cannot be simply represented by dimensional expressions of the traditional type, and can only be simply represented by an appropriate dimensionless expression, are *structured transformations* (of energy, atomic systems, etc.).

The next step should be the physical interpretation of the diagram and its generalisation to cover more complex situations. This may require a generalised concept of one-way processes which is developed in a paper complementary to this. A subsequent paper will seek to combine their results.

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THROUGH THE DEN OF THE METAPHYSICIAN *

WARREN S. MCCULLOCH

WE are again in one of those prodigious periods of scientific progress—in its own way like the pre-Socratic period to which we are still indebted for the crisp formulation of our physical problems and, consequently, for our epistemological quandary. Anyone who has had the good fortune to listen to Wiener and von Neumann and Rosenblueth and Pitts wrestling with the problems of modern computing machines that know and want, has a strange sense that he is listening to a colloquy of the ancients. But they would be the first to tell you that they themselves are drunk with an American wine of an older vintage ; they quote liberally from Charles Peirce and from Josiah Willard Gibbs. These men have altered our metaphysics by altering our physics. It is epistemology that is most affected, for it is the physics of communication which is today receiving an adequate theoretical treatment. For the first time in the history of science we know how we know and hence are able to state it clearly.

Physiologists, working on the central nervous system, have long had such a goal in mind. Rudolph Magnus, inspired by Immanuel Kant, made his last great lecture one on ' the physiology of the *a priori* ' by which he meant how those mechanisms work that determine for us the three-dimensional nature of our world, its axes and its angles, and that give to us our sense of velocity and acceleration, from which he held our notion of time to be in large measure derived. Perhaps the most notable attempt of this sort was by Sir Charles Sherrington, entitled *Man on his Nature*, for, near the end of a life spent on studying the ways of the brain, he was forced to the conclusion that ' in this world, Mind goes more ghostly than a ghost '. The reason for his failure was simply that his physics was not adequate to the problem that he had undertaken. That has so regularly been the shortcoming of scientists who would have approached this problem, that even Clerk Maxwell, who wanted nothing more than to know the relation between thoughts and the molecular motions of the brain, cut short his query with the memorable phrase, ' but does not the way to it lie through the very den of the metaphysician, strewn with the bones of former explorers and abhorred by every man of science ? ' Let us peacefully answer the first half of his question ' Yes ', the second half ' No ', and then proceed serenely.

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Our adventure is actually a great heresy. We are about to conceive of the knower as a computing machine. That is not a new heresy. It has already been prejudged by Dryden in *The Hind and The Panther*, when he says,

> And if they think at all, 'tis sure no higher
> Than matter, set in motion, may aspire.

I believe that he is correct, only I am not sure that that may not be high enough. I have no intention of burdening you with the detail of the construction of the computing machine, whether these be man-made or begotten. The latter are my daily business. My problem differs from that of the men who build computing machines only in this—that I am confronted by the enemy's machine. I have not been told and must learn what it is, what it does and how it does it. It is a complicated computing machine consisting of 10^{10} relays. Each of these relays receives signals from other relays. Each on receipt of an appropriate signal—or group of signals—emits a signal. It is my business to learn how these relays are connected one to another, what it takes to fire a given relay, how long after receipt of a signal it will send a signal, and how a signal received can prevent a relay from responding to a second signal otherwise sufficient.

But we must first be prepared for the kind of world we now invade. It is a world for Heracleitus, always ' on the move '. I do not mean merely that every relay is itself being momentarily destroyed and recreated like a flame, but I mean that its business is with information which pours into it over many channels, passes through it, eddies within it and emerges again to the world. Surely Heracleitus would feel at home with such a knower. Paradoxes raised by such a conceit of the world have always led, through Parmenidean unity and Eleatic riddles, to a Democritean multiplicity ; that is, to one in which the stuff of the world is a set of atoms—of indivisibles —leasts—which go batting about in the void. Whenever they make their appearance they bring with them Chaos. Therefore, it may at first sound paradoxical that every modern computing machine of any great size or scope works in a Democritean manner. Again, I do not merely refer to its being constructed out of chemical atoms, but I mean that it is a machine with a least count. Its signals are quantised. Each either happens or does not happen. It does not half happen. To this general rule, the nervous system is no exception. Its least signal, or nervous impulse, is an all-or-none event. If a neuron emits a signal, it does all that it can then do. Thus, not merely is the structure of the nervous system quantised in neurons, but its action is quantised in their impulses, or least signals. Surely Democritus would claim kinship with a knower whose actions were thus atomic—perhaps more readily because there is at present no reason to suspect that these atomic signals differ significantly save as to when and where they occur. Hence, all that a least signal can say to the next relay is that the relay that emitted that signal had been adequately excited. From this it follows that whatever quality may be anywhere detected in the universe by our knower, must depend upon the *figure* of these least signals in time and space. Moreover, since every relay has a characteristic delay, given enough relays it is always possible to convert a figure of impulses given simultaneously in space into a figure of excitation in time over a single relay, or *vice versa.*

Consider any one relay. At any given time, say a millisecond, it can be in either of two states : it can be transmitting one signal or none. Hence, two independent, or unconnected, relays can be in any one of four states ; three in one of eight, four in one of sixteen, etc. That is, of n neurons, the number of possible states is 2^n. It takes one signal, or one unit of information, to determine in which of two states any one relay is in any one relay time. Now if we have a single relay but consider it at two times, then it can be, in the two times together, in any one of 2^2 states ; in three times in 2^3 states ; in four times, 2^4 states and so on. Clearly the same amount of information can be conveyed by n independent relays in one unit of time as can be conveyed by one relay in n units of time. Hence, if we desire to convert a given figure at one time into a series in time, we need as many units of time as we had independent relays. Note that the unit of information appears with a negative sign in the exponent ; that is, given n independent relays, or one relay at n times, one unit of information, by fixing the state of one relay, subtracts one from n, leaving 2^{n-1} states possible.

The *a priori*, or logical, probability that a neuron is in a particular state at a particular time is one-half ; that two are in a given state, one-fourth and so on. Hence, information is exactly the logarithm to the base 2 of the reciprocal of the probability of the state. But this has a peculiarly familiar sound. Gibbs had defined entropy as the logarithm of the probability of the state. In Wiener's phrase, entropy measures chaos and information is negative entropy. So, corresponding to the second law of thermodynamics, that entropy must always increase, we can write for any computing machine the corresponding law—*information can never increase*. That insures that no machine can operate on the future, but must derive its information from the past. It can never do anything with that information except corrupt it. The transmission of signals over ordinary networks of communication always follows the law that deduction obeys, that there can be no more information in the output than there is in the input. The noise, and only the noise, can increase. Therefore, if we are to deal with knowers that are computing machines, we can state this much about them. Each is a device, however complicated, which can only corrupt revelation.

In order to preserve a correct sense of proportion, let me be technical for a moment. The human eye has about one hundred million photo-receptors, whereas it has but one million relays to carry that information to the brain. The rest of the body contributes another million channels. Thus we may figure approximately three million relays putting information into the nervous system simultaneously. Let us next evaluate the output of the nervous system. To do so we have conceived a piano player performing at top speed, given him a keyboard of 100 keys, let him strike 10 times per second with each of ten fingers with any one of ten strengths and let his hands each span ten keys. No man can do so much. Yet, when we translated this into the number of all-or-none signals per second, we discovered that it was only three units of information per millisecond.

Today we can estimate the amount of information conveyed by sounds, speech or music, for there is a device which samples the sound every millisecond and sends, at most, three all-or-none signals according to the instantaneous amplitude of the wave. These three decisive signals per millisecond convey the full information, for waves reconstructed from them are indistinguishable from the original sounds.

Thus the overall reduction in information from input to output of brain is of the order of a million to one if we neglect the eyes proper, and a hundred million to one if we include them. What becomes of all that information ?

In large measure we use it this way. It is easy to have a relay which will fire only if impulses from two sources arrive almost simultaneously. Such a neuron detects the coincidence of information over the two channels to it, and it responds only in that case in which they agree that something happened. Therefore, the logical, or *a priori*, probability of finding an impulse in it is the product of the probabilities of finding one in each of the afferent pathways singly. That is to say, it is more *improbable*. The chief reason for the enormous reduction from afferent signals to efferent signals is the requirement of coincidence along the way. Every such requirement of coincidence, by reducing the *a priori* probability of a signal in the output, increases the assurance which can be placed in any subsequent signal, for that signal must then be due to coincidence in the world impingent upon our receptors. In short, by throwing away all information that fails to agree with other information, we achieve an immense certainty that what we do observe is due to something in the world. Clearly, we should follow the same procedure in forming our hypotheses. Each should be of minimum logical, or *a priori*, probability so that if it be confirmed in experience then this shall be because the world is so constructed.

At this point I should mention that the limitation of the information in the output is in large measure determined by the effectors themselves. This is the ineluctible corruption of thought in deed. As there remain 10^{10} neurons, or relays, in the central nervous system, it is obviously impossible for any man ever to convey so much as one part in 10^{10} of what is going on in him. Even though he be a poet, the rest of his soul remains his private property. Now we habitually think of our sensations, or any knowledge of the world so derived, as an activity going on in those places in which afferent channels end, but we can only demonstrate it by output over some efferent channel. Consequently, the answer to any particular question, as to whether we did or did not know something, will be dependent upon the particular efferent channels we choose to examine.[1]

With all of these limitations and hazards well in mind, let us ask

[1] This is painfully obvious to any one who deals with diseased brains, for it is often a matter of diagnostic or forensic importance to know whether a man is or is not conscious of his deeds or of things about him. Conscious in this context means that he can, then or later, bear witness of those events of which we can also bear witness. In malignant stupors the patient is conscious, in benign, unconscious, but we can only determine this when he is no longer stuporous. The epileptic who is unconscious of his acts is never responsible for them at law although the acts are so complex and so adjusted to the world about him as to insure that he is then and there aware of much related detail in what is then going on.

whether a knower so conceived is capable of constructing the physics of the world which includes himself. But, in so doing, let us be perfectly frank and admit that causality is a superstition. By causality I mean any law of necessary connection between events.

Let me put that this way, in the old quillet. Nothing is true and not true, nothing false and not false, nothing true and false. Hence, whatever is is either ' true or else false ' ; or else it is ' neither true nor false.' In the first case it is a proposition, in the second case it is not. It is then impossible truthfully to deny that propositions exist, for either the denial is a proposition, in which case it is false, or else it is not a proposition and, hence, neither true nor false. Yet, that it is impossible to deny its existence truthfully is no assurance that any proposition exists. It remains an assumption that propositions exist. Later it will be apparent that even that assumption is inadequate. If there is to be in the world any knowledge about the world, there must be true propositions about the world. In the world of events a true proposition is an event which materially implies another event. That is, in the simplest case, one which happens only if that other event happened. In the world of physics a true proposition implies what it asserts.

The world of physics is fairly described by Whitehead's ' Aether of Events '. The world for him is the whole that only happens once ; and to be an event is to be some part of that whole. In his description of the relation of whole to part, there are, however, two assumptions which we have ignored in our theory of the action of the computing machine, and it is these assumptions which permit of analytic continuation, a going toward continuity. He supposes that there can exist an event which has no least part, and that if A is a part of C, then there is always an event B, such that A is a part of B and B is a part of C. Apparently a computing machine, quantised as ours, is not built on either of these assumptions. We have supposed a least signal, that is to say, a signal which either occurs or does not occur. That least signal is a proposition ' on the move '. It is true or else it is false and it occurs at some particular time and at some particular place. Since the number of relays is finite, these can be ordered and a number assigned to each, and since we can quantise time in units equal to relay time, we can start counting at any particular instant and, by subscripting the number of a relay by a number representing the time of a signal there, we can construct statements in which the signal of a given relay is expressed in terms of those signals which reach it. The resultant calculus is the calculus of atomic propositions of Whitehead and Russell, subscripted for the time of the occurrence of the propositions. Each of these signals is, strictly speaking, an atomic propositional event, which can only be or not be, and, if it is, may be either true or false. Each materially implies its proper antecedents.

Consider for a moment a computing machine in which there are no closed paths, that is, no circuits around which signals may chase their tails. In such a system each signal, implying its antecedents, implies a signal of a relay nearer to the receptors until we arrive ultimately at them. Their signals likewise imply the world impingent on our sense organs. In the strictest sense of the word what goes on in such a nervous system implies the world impinging upon its sense organs. But note that the domain of implication extends only backward in time. Even if the threshold of every relay were fixed, between the moment at which a given set of impulses started from a spot in our brains to our hands and feet, there might intervene other impulses coming, by shorter paths from the outside world, to our effectors. Thus, in the forward direction, the relation falls short of implication in so far as aught else intervenes. In short, our thought does not imply our action but, as we say, only intends it. Perforce we distinguish between futurity and intention. Our notion of our wills has, I believe, arisen from this enforced distinction, and its perennially questioned ' freedom ' presumably means no more than that we can distinguish between what we intend and some intervention in our action. If I shall do what I will do then my will is free.

To make this clear, let me return to the quillet. Even an atomic signal asserts that such and such is the case and it is true only if such and such is the case. If we assume that there are such things as true atomic propositions of the kind called signals, then these are significant propositions as Wittgenstein uses the term ; for the truth or falsity of each signal depends upon whether or not that which it asserts occurred. If significant propositions of this kind are to be true there must be a law of necessary connections between the event which is the proposition and the event which it proposes. But a law of necessary connection among events is causality. In such a world the superstition of causality must first be assumed by anyone who wishes so much as to deny it. But, if we have once admitted causality in this sense into the workings of our brains, our significant propositions are determined by our past, and freedom from the past would make no sense. We really want freedom toward the future—freedom from affairs intercurrent between our ideas and our deeds. This is all that is needed to fix our responsibility for those deeds.

Among those deeds are our words, and every word has the flavour of being given 'once for all'. It bespeaks a universal, an idea or quality, appearing for us in the world. In order that the activity of a nervous system should imply a universal, or idea, we need not merely the calculus of atomic propositions but also logical quantifiers which assert that *all* or *some* *x*'s are such and such ; and the question at once arises how these are introduced. Again in Whiteheadian phrasing, the problem is not one of how we apprehend events but of how we recognise objects. For me, the most difficult part of Whitehead's theory of percipient events is concerned with the notion of primary recognition. How can we come by an idea we did not have before ? You will notice that, had our nervous system no closed paths, a signal anywhere within it would mean that something had impinged on a receptor at some particular millisecond prior thereto. *Per contra*, if there exist closed paths around which signals may reverberate, and if the sequence of these signals be patterned after some fact, then the pattern persists in us as long as the signals continue to reverberate. Each time they circle they literally know again, or re-cognise, that which was given but once in their input. This is memory of a kind and it suffices to free the signals of their reference to a particular time. That circuit knows that such and such happened at *some* previous time but not at *what* time. This introduces the existential operator for time, namely there was some time such that at that time so and so happened. Notice that the tense is past.

Not all human memory is of this kind. While we are young we may grow new connections. By means of them the ways that led us to our ends become embroidered into the warp and woof of our nervous net, but no kind of memory can do anything which cannot also be done by activity circling in closed paths. Pitts and I have already demonstrated that a machine constructed with such closed paths, as well as open afferents and efferents, can produce any consequence which can be produced from its afferents, that it can compute any computable number, or arrive at any conclusion which follows logically from any finite set of premisses. Moreover, to satisfy any doubting Thomas who would thrust his electrodes into the brain, we have shown that any wound or any other alteration of the net, say one acquired by use, can be simulated by the action of a hypothetical unalterable net of unalterable neurons. But injury and learning will differ in one respect ; for, in the case of learning, the known afferent channels suffice ; whereas, in the case of injury, we require a new afferent channel to initiate an action in our hypothetical net.

Conceive, then, our knower as an unalterable net of unalterable relays. To this net come impulses from the world and from it impulses go to the world. Within it are not only thoroughfares but circles. Of these we have mentioned only one that will reverberate. It suffices to free us from one particularity, reference to one past time, but there are other closed paths which are important in our knowledge of universals. Their action was well described in 1817 when Magendie defined what he called the 'reflex' as an activity which began in some part of the body, passed by way of nerves to the central nervous system, whence it was reflected to that same part of the body where it stopped or reversed that process which had given rise to it. Electrical engineers refer to such a circuit-action as inverse, or negative, feedback. The anatomy of some of these negative feedbacks was exhibited in 1825 by Sir Charles Bell. The mathematics for handling many of such actions is to be found in Clerk Maxwell's paper 'On Governors', given before the Royal Society in 1868. Their properties are well known. Every such circuit pulls toward some particular value of some variable. In the case of reflexes, something, say the length of a muscle or the temperature of the body or the pressure in the artery, is measured by a set of receptors which send impulses to the central nervous system, whence impulses are sent back to those structures to bring them back to the particular state established by the reflex arc. This particular state measured by those receptors is the goal, or aim, or end in and of the operation of that reflex. By means of these reflexes we achieve dynamic stability in a changing world.

One such circuit frees us from the variations in intensity of stimulation and consequent variation in the number of impulses that would otherwise excite the bark of the brain, called the cerebral cortex. This circuit, unlike the reflex path, lies entirely within the brain. Impulses ascending to the thalamus, or last relay beneath the bark, are passed on to the cortex and from portions of the cortex descend, by a devious path, to the thalamus where they inhibit the relaying of impulses to the cortex, thus keeping the cortical activity at a nearly constant level. This is an 'automatic volume control'. It leaves us with a figure of excitation in the cortex which is invariant under fluctuations of the intensity of peripheral stimulation, making it possible for us to determine some aspect of that stimulation regardless of intensity. Hence we detect that there was some intensity of stimulation which was of such and such a figure. In short, there is done for us with respect to intensity what simple reverberation did with respect to time.

Let us next consider the so-called appetitive circuit. It is an inverse feedback over a path which extends beyond the body. In the external part of the path is to be found the goal, or target, and the circuit is said to be 'inverse over the target'. Today one of the best known of these circuits runs through the eye and the brain-stem. It turns the eye automatically toward any object which appears in the periphery of the visual field. By turning the eye so as to bring the image of the thing seen to the centre of the receptive surface of the eye, it translates an apparition to a standardised position and hence its projection by means of signals to a standardised place in the bark of the brain. In so doing it rids the form to be seen of the gratuitous particularity of that position at which it happened first to appear. Because for us the centred apparition established by this reflex is a given one from among the many possible exemplifications, we refer to the process as that of reduction to the canonical position. Every reflexive circuit brings some apparition through a series of positions, intensities or whatnots, to the final canonical one of all the many through which it has progressed. So, in mathematics, if we be given the Pythagorean theorem we reduce it to the canonical form of the axioms, postulates and definitions at the beginning of the book. Having chased it thus back to the cave of the sun, we cry, 'Ah-ha', for we have recognised it.

We pass now to another way of securing universals. At first sight it looks altogether dissimilar. It is, in fact, obviously a method of averaging. The epicritic modalities of sensation, for example vision, map the impingent activity of the world on a fine mosaic of cortical relays. The centred form, given by the reflex of the eyes, appears in the input to the visual cortex as the distribution of impulses over a fixed area and about a fixed centre. In this portion of the cortex of the brain are actually made all of the possible dilatations and constrictions of the forms impinging upon it. They are limited by the grain of its mosaic. These are then added and relayed, as a set of averages to the next portion of the brain. Clearly those averages do not depend upon the size of the particular object that was seen. The image may have been big or small, but since from any given one of them we made all sizes, the size of the original does not affect the averages. The output is size-invariant. This size-invariant has been abstracted from the entire group of dilatations and constrictions and corresponds to the shape regardless of the size. As the former method conforms to Plato's notions as to the origin of ideas, so this recalls Aristotle's notion of the abstraction of ideas from many particular exemplars. Those who desire a clear and rigorous statement of this process will find it in the *Group Theory and Quantum Mechanics* of Weil.

Let me oversimplify that statement. We have obtained a group invariant as follows : We have calculated a set of numbers, each of which is, for all transformations belonging to the group, the average of numerical values arbitrarily assigned to the presence or absence of signals at particular points or at particular times. That in which one mechanism obtaining these invariants, or universals, differs grossly from another mechanism lies in that arbitrary manner in which the numerical values are assigned. For example, in the case of the centring reflex, the value *zero* is actually assigned to all translations of the apparition until it is centred. Whereas, in the case of the dilatations and constrictions in the visual cortex, all evidence points to the value 1 being assigned if a cell situated there was fired then—otherwise zero. We have, then, in the general statement all ways of obtaining universals. Their differences are accidental, and depend upon the arbitrary way in which the mechanism assigns the values. At this level, Aristotle's method and Plato's method are peas in the same pod.

There is, of course, *one* limit to the number of times this process can be repeated upon the output of a previous structure. We have only a finite number of relays and finite span of life measured in their unit times. Short of that limit we are, of course, at liberty to have the idea of ideas and the idea of the ideas of ideas, etc. In short, our knower is, in the Spinozistic sense, conscious. Also, he may discover himself among things known to him in his world, and be self-conscious in exactly the same way in which he is conscious of anything else in his world. But all of his ideas, no matter how high-flown, are ultimately reducible to a logic of the lowest level and, even at that lowest level, to a finite number of atomic propositions. He can no more know the infinite than he can know the future. Of course, he may guess the future. He has only to run a correlation (over time) of past events, abstract a universal and project it upon the future. The projection is always but a guess. The best guess may go wrong. The scientist knows this to his cost ; for every hypothesis is a guess as to the outcome of an infinite number of possible experiments. We expect that every hypothesis will be disproved. Surely none can be proved. This is, in fact, their glory ; for we know, *when we have* proved them false, that they *were* significant propositions.

To have proved them false is indeed the peak of knowledge, for we extrapolated them out of atomic propositions in the input to our calculating machine. These signals in the input are independent propositions. Any one can be false or true and the rest remain the same, be they true or false. Of no one of them can we ever know that it is true or false. These are merely revelations and we cannot look the giver of the data in the teeth. How, out of such data, finite and discrete, have we been able to construct the notions of analytical continuity which are so clearly requisite for the Whiteheadian analysis of the physicist's world ?

The problem of topological closure is brought up in the paradoxes of Zeno of Elea. We require a meaning of ' in-between ' quite different from that given in any single experience. The world given in experience is always ' this ' up to where it is ' that '. The ' continuum of sense-awareness ' is not continuous in the mathematical sense. Where we have no receptor we pick up no impulse. That point has no connection to the cortex. It is not represented directly by any impulse arriving there. We are not directly aware of our ignorance. For a moment conceive the same portion of the world given in two of these speciously continuous modalities of sensibility. In each the world appears to be continuous. But there is no reason why that which is continuous in the one modality should fit that which is continuous in the other modality—no *a priori* reason ! Most of the time the continua fail to be congruent and any circuit, subsequent to these recipients, which can detect the failure of congruence will generate a meaning of in-between. This meaning of in-between is just as good a universal as any other ; for it we can construct an invariant. We have but to give it voice and, like God or a baby, cry ' do it again, do it again, don't stop ', and we have generated the fundamental notion required for topological closure. For the paradoxes of Zeno of Elea, seen motion appears continuous through seen space likewise continuous. We have but to abstract the required universals and we are able to propose the paradoxes.

I do not believe that anything more is expected of us. A stick thrust in water felt straight and looked bent to a Greek. The sun moved for the inquisition, the earth for Galileo. Light is a wave for Schrödinger and a particle for Heisenberg. But even the last have had their Dirac. The seeming contradictions vanish in the grace of greater knowledge. We have learned that the answer depends upon how we ask the question. And we have learned to ask the question so as to get an answer of a kind that we can use. Knowledge itself presented no great problems to the Greek mind until it had invented its theoretical physics in terms of insensible reals. It was these that bothered Galileo and Descartes.

To suppose the world continuous instead of discrete did not help Descartes. He was willing to admit that an automaton might be constructed which could do everything that a man does. Yet, to this automaton he would deny Mind. Doubting, knowing, thinking, seemed to him beyond its scope. Now that we have constructed automata, which, like us, can compute any computable number, can formulate clear ideas and, by inverse feedback, have purposes of their own, built into them as ours are born in us, we are confronted with the humbling prospect of the work of our own hands—machines more steadfast in their purposes, more supple in the execution of these purposes and in their modifications for good cause, capable of learning and thinking far beyond us, at present in certain fields only, but, in time to come, in any field for which we care to construct them. As yet we have not made them capable of multiplying their kind. That would be for us the final mistake.

In closing let me remark, I am not using knowledge in any restricted sense. The propositions with which I have concerned myself are the significant propositions of machines, those that propose something external to the event which is the proposition. But if instead of relays that wait for signals to trip them, I install in any computing machine a relay which will fire itself each millisecond, I can introduce into it all tautological propositions. To my mind they fall short of being knowledge, which I look upon as an activity that says ‘ such and such is the case ’ and such and such is the case ! These propositions are primary and atomic. We cannot know that we know them. A lie is as truly a proposition as is a truth, and their independence prevents any test of truth. In hypotheses, proved false, we are at least aware of the error of our ways. For a scientist that must be sufficient. If we cannot rest content with our brains we can at least construct machines which, like them,

If they think at all 'tis sure no higher
Than matter put in motion may aspire.

I am convinced that that will be sufficient to guide me through the very den of the metaphysician, strewn with the bones of the former explorers. One of these is surely the femur of Immanuel Kant—his confusion of the empirical with the epistemological ego. This supported him on the solid ground of science while his skull was highest in the realm of theory. Another is certainly his skull which housed his computing machine, for the net of his relays embodied his ' synthetic *a priori* '. If my bones are to fall beside them, I hope aftercomers will recognise my spine. Its joints are the superstition of a necessary connection between events—called causality. I humbly submit that it is but a reincarnation of Saint Thomas' faith that God did not give us our senses to fool us. It is enough that this trust in the goodness of God cannot truthfully be denied. So, at least in some places, the den of the metaphysician seems curiously like the cave of the sun, and hence like home.

CREATION AND THE "NEW" COSMOLOGY *

MILTON K. MUNITZ

I

IN the recent, much-publicised cosmology of 'continuous creation'[1] a number of issues of specifically philosophical interest are raised, whose resolution proves essential for an understanding and evaluation of the theory as a whole. These issues revolve about the precise *meaning* to be given to the concept of creation as it functions in the theory and the *methodological grounds* offered by way of justification for its use. Accordingly, whatever might be the fate of the theory as it undergoes the ordinary checks of mathematical analysis and observational verification, I shall argue that much of the philosophic (sometimes euphemistically called 'aesthetic') commentary provided by its authors exhibits a number of difficulties and obscurities that stand in the way of its total acceptability as formulated at present.

2

The essential novelty of the theory consists in the suggestion that matter is being created in all epochs and throughout space at a statistically uniform rate which is sufficient to compensate for the continuing expansion of the universe, and thus to maintain the universe in a steady-state (in a generalised hydrodynamic sense) and at an over-all constant density. The expansion is inferred from the observed red-shift in the spectra of galaxies, which is interpreted as due to their recessional motion. Matter, it is proposed, is being created in an elemental form at random throughout space in a way which is independent of the matter already existent in various stages of agglomeration. The calculated rate of creation is roughly the mass-equivalent of one hydrogen atom per litre of volume every billion years. It is accordingly a process which, because of its virtually

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[1] Also known technically as the 'steady-state theory of the expanding universe'; the original papers are: H. Bondi and T. Gold, *Mon. Not. Roy. Astr. Soc.*, 1948, **108**, 252; F. Hoyle, *Mon. Not. Roy. Astr. Soc.*, 1948, **108**, 372; F. Hoyle, *Mon. Not. Roy. Astr. Soc.*, 1949, **109**, 365; cf. W. H. McCrea, *Endeavour*, 1950, **9**, 3; H. Bondi, *Cosmology*, Cambridge, 1952, Ch. 12.

infinitesimal proportions, is incapable on practical grounds of direct confirmation. Through a gradual process of condensation and accretion, conglomerate macroscopic bodies are eventually built up to the enormous proportions of galaxies and clusters of galaxies. At any given time, within a sufficiently large volume of space, the number of galaxies remains constant since new ones are continually being formed to replace those disappearing over the 'horizon' of the observable universe. The theory allows for the idea of a universe whose existence in time is infinite both in the past and the future, and whose existence in space is likewise infinite in extent though the range of observability is set by the limiting velocity of light.

3

It is claimed for the concept of creation as it figures in this theory that it is wholly removed from the intellectually suspect domains of metaphysics and theology and now finally established within the domain of scientific accuracy and comprehension. On earlier theories

> it was believed that the creation process occurred at a definite ascertainable epoch in the past. Properly interpreted this point of view sidesteps the whole question; for unless we say *how* the creation occurred, nothing has been achieved. Indeed the word 'creation' in this context was simply a device for terminating the discussion as soon as an awkward question had been posed.[1]

With the steady-state theory

> the problem of the origin of the universe, that is, the problem of creation, is brought within the scope of physical inquiry, and is examined in detail instead of, as in other theories, being handed over to metaphysics.[2]

With respect to these claims, however, it is important to keep two points clearly distinguished. One has to do with the consequences or effects of matter when 'given' as existing, the other has to do with the possible antecedents or causes which bring it into existence. In discussing creation, the authors of the steady-state theory believe it sufficient to explore the former without any attention to the latter. It is here that there is to be found one crucial philosophical difficulty in their account. Thus one set of questions has to do with the specific properties which matter is said to possess when it appears or, allegedly, is created : properties such as spatial distribution, rate of appearance, initial velocity, ' temperature ', atomic structure and the like. This is what is referred to as the 'physics ' of creation. It is the filling out of these details, the drawing of their consequences, i.e. the linking of them with the facts of astrophysics, atomic theory, and observational astronomy, which underlies the claim that creation is therewith brought within the scope of scientific understanding. And there can be no doubt that progress in this direction makes for the kind of deepening and broadening of insight which is characteristic of science. *Yet the possible success of this line of inquiry would leave completely unaffected the crucial claim that what has been investigated is matter which has been created.* Those theories like Lemaître's or Milne's which posit a point singularity in the finite past, identified as Creation, are actually in this respect not at all inferior to the steady-state theory. For they, too, undertake to give us what may be regarded as the ' physics ' of creation, i.e. specifications as to initial motions, material composition and the like. The complaint that they locate the creation at a singular point in the past, thus making it inaccessible, is justified if it emphasises that the creation *process* is left shrouded in mystery. Hoyle, therefore, is quite correct when he remarks that

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[1] Hoyle, *New York Times Magazine*, 1st June, 1952, p. 12

[2] Bondi, *Cosmology*, p. 140

> it is against the spirit of scientific enquiry to regard observable effects as arising from ' causes unknown to science' and this is, in principle, what creation in the past implies.[1]

But exactly the same criticism applies to the continuous creation theory. By spreading creation out in time and space, there is no reduction in the mystery, since multiplication of the occasions of creation as contrasted with the single unique event leaves it open to exactly the same objections as the latter.[2]

Hoyle, in particular, would argue on what he takes to be pragmatic grounds that all we need concern ourselves with is the ability of the theory to make successful predictions, its ability to work well.

> We do not ask [he tells us] ' Where does gravitation come from ?', or if we do, science supplies no answer. Or again, we do not ask, ' Why do electric and magnetic forces occur in nature ?' Instead we ask the question ' How does gravitation operate ?' ' How do electric and magnetic forces operate ?' Science does not seek to justify the

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[1] Hoyle, *Mon. Not. Roy. Astr. Soc.*, 1948, **108**, 372

[2] cf. H. Dingle, *Scientific Adventure,* London, 1952, p. 166

> existence of gravitation and electromagnetism. What science does say is, ' If gravitation exists, then it works like this . . .' or ' If electricity exists then it works like this . . .' Exactly the same situation applies to the creation of matter. We cannot say why matter is created or where it comes from, but we can say ' If matter is created continuously then it is created in such and such a way.'[1]

It is true, of course, that scientific explanations do not undertake to give justifications in the above-intended sense, and in this sense it would be unwarranted to ask for the ' purpose ' of creation. It is also true that in speaking of ' gravitation ', ' electricity ' and ' magnetism ' all that is effectively involved for physics is bound up with the equations expressing these ideas, the rules according to which these are to be interpreted and their capacity to link known facts or predict fresh ones in a satisfactory way. If we would extend, however, these same ideas to the present theory, it would at best direct our estimation of its worth to an examination of what the equations contain, the various properties assigned to matter and the way we are enabled on the basis of these formulae to systematise our subject matter. It would, however, *not* justify the conclusion that matter has been ' created in such and such a way '.

4

In saying that matter is created, the authors of this theory are explicitly clear about the fact that it is an *ex nihilo* process. Bondi writes :

> It should be clearly understood that the creation here discussed is the formation of matter not out of radiation but out of nothing.[2]

Hoyle says :

> From time to time people ask where the created material comes from. Well, it does not come from anywhere. Matter simply appears—it is created. At one time the various atoms composing the material do not exist and at a later time they do.[3]

Finally, McCrea, in expounding the theory asserts :

> there can be no causal treatment in a physical sense of true creation. This is almost a matter of definition. If the creation of matter is caused, as is conceivable, by existing physical conditions, then the true creation

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[1] Hoyle, *New York Times Magazine,* loc. cit.

[2] Bondi, *Cosmology,* p. 144

[3] Hoyle, *The Nature of the Universe,* Oxford, 1950, p. 125

> is of those conditions, and we should not try to give a physical theory of that creation. But we are here regarding the creation of matter as itself spontaneous, i.e. as something 'given' and not to be treated causally.[1]

Now to claim that matter 'simply appears', that it is a 'formation out of nothing', that its appearance is 'not to be treated causally', is simply to deny the *possibility* of achieving any kind of scientific explanation of its appearance. This is, however, clearly a species of dogmatism, the irrevocable claim to an *ignorabimus* which is incompatible with the spirit and method of scientific inquiry. It is one thing to say that we don't yet understand how a process takes place, even though we might adduce reasonable grounds for affirming the existence of the process. It is an altogether different matter to say that we shall never understand its mechanism. The latter violates a primary rule of science, as Peirce expressed it, 'not to block inquiry'.

Suppose that indeed it were established in some way that matter does appear in the manner and with the various properties as claimed. We should then be able to speak of this as a 'law of nature' in one sense of that phrase, namely, as a regularity found to hold in fact. But it is precisely as a regularity that one would look for its explanation. And this criticism holds regardless of the particular philosophy of science one adopts. It will commonly be granted that the primary task of science is the discovery of laws. On one account, such laws are basically generalisations. Even where science advances to the level involving the establishment of an intricate deductive system or logical hierarchy of such laws, those which are in a given system the premisses of the system are still essentially statements of regularity.[2] Another view takes the primary technique of scientific explanation to consist in the application of laws or theories regarded as modes of representation of phenomena and involving characteristic techniques of calculation or rules of inference, while not themselves being facts or generalisations of facts.[1] On either view science always leaves the way open for more adequate explanations, and does not regard any given explanation as final. If we proceed by looking for more and more inclusive generalisations, then no given regularity (generalisation) is ultimate, i.e. in principle unexplainable; the demand always exists for finding a more inclusive generalisation under which a given one may be subsumed, i.e. deduced and so explained. And if we consider the primary technique of explanation to reside in the use of laws or theories interpreted as essentially techniques of inference, then, confronted with a hitherto unexplained regularity, it will be the objective of inquiry to find a theory whose way of looking at phenomena and whose mode of operation will enable the regularity to be understood. (Here, the question as to what 'explains' the theory is a meaningless one, since theories are not like generalisations and are not explained by subsumption under 'wider' theories. At best the rules which characterise one theory are incorporated, i.e. added to other rules which yield a more refined theory.) Given, then, the statement of the regularity with which matter appears, there is no reason, no methodological warrant, for insisting that this is itself an ultimate fact, capable of explaining other facts but incapable of being explained itself.

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[1] McCrea, *Endeavour*, loc. cit., 7

[2] e.g. a recent statement of this view: 'To explain a law . . . is to incorporate it in an established deductive system in which it is deducible from higher level laws. To explain these higher-level laws is to incorporate them and the deductive system in which they serve as premisses, in an established deductive system which is more comprehensive and in which these laws appear as conclusions. To explain the still-higher-level laws serving as premisses in this more comprehensive deductive system will require their deduction from laws at a still higher level in a still more comprehensive system. At each stage of explanation a 'Why?' question can significantly be asked of the explanatory hypotheses; there is no ultimate end to the hierarchy of scientific explanation, and thus no completely final explanation.' R. B. Braithwaite, *Scientific Explanation*, Cambridge, 1953, p. 347.

A confirmation by way of illustration of the above general criticisms is to be found in the recent work of McCrea and McVittie.[2] Both seek, on the basis of the main ideas of the steady-state theory, to find some way in which the creation process can be made genuinely intelligible—and thus not a creation process at all. The basic suggestion put forward by McCrea and worked out in a slightly different manner by McVittie is that on the basis of relativity as distinguished from Newtonian theory, one can look for a connection between stress and the 'creation' of matter. With a negative stress as made possible in relativity theory, the creation of matter becomes the mass-equivalent of work done by this negative stress in the expansion of the universe. With the possibility as allowed under relativity views of the conversion of stress into mass and *vice-versa*, the creation of matter is no longer an

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[1] cf. S. Toulmin, *The Philosophy of Science*, London, 1953, pp. 42, 84 ff.; W. H. Watson, *On Understanding Physics*, Cambridge, 1938, pp. 52 ff.

[2] McCrea, *Proc. Roy. Soc.* (A), 1951, **206**, 562; G. C. McVittie, *Proc. Roy. Soc.* (A), 1952, **211**, 295

ex nihilo process. From a methodological point of view (whatever might be the technical value of these suggestions) one sees here at work the scientific mind typically engaged in finding connections rather than showing its willingness to accept some fact as inexplicable.

5

It may be said, however, in rejoinder, that to refer to the possibility of subsumption under ' wider laws ' or ' incorporation within a wider theoretical framework ' is precisely to miss the whole point in speaking of creation at all, since whatever may be the case in other situations, here one intends by the very employment of this concept to underline the point that we are confronted with an ultimate fact, itself capable of serving as a fundamental premiss or rule of inference but, by the very fact of its being ultimate, incapable of inclusion within any wider or more basic framework. That this is an error has just been argued on the ground that there is no warrant for taking scientifically any premiss or rule as *absolutely* basic or unique. Further, as I shall now argue, the very use of the term ' creation ' in such an allegedly ultimate premiss or rule is vacuous. This has the result that it leaves the premiss or rule itself crucially indeterminate. As we have seen, there is an essential difference between the following two statements : (1) matter in an elemental form and with the various other properties as specified by the theory *is found* in the universe, and (2) matter in an elemental form and with the various other properties as specified by the theory *is created* in the universe. The authors of the theory insist on taking the second expression as the correct one, but whatever strength there is in the content of their proposals or methodologic soundness in their procedure lies in actually using the first expression. To say that matter is found in the universe leaves open the possibility of explaining its appearance, whereas to say it is created not only denies such a possibility but also *employs a term without any significant content.*

> I have often been asked [Hoyle tells us] ' Where does the newly created matter come from ?' This is also a meaningless question. [Like the question about the origin of the universe as a whole, according to him.] It is only because in everyday life people have got used to the idea that matter must be conserved. When a conjuror pulls a rabbit out of his hat we know that the rabbit did not suddenly come into existence at the moment we see it and therefore it makes sense to ask ' Where did

> the rabbit come from ?' But if the rabbit were indeed created by the conjuror, it might make no sense at all to ask this question.[1]

To which we must reply that, on the contrary, whether presented with rabbits or particles of matter it *does* make sense to ask ' Where did they come from ? ' To be told that where these are created it makes no sense to ask this question is *to beg the question*, to assume the meaningfulness and legitimacy of referring to the process as one of *creation.* Now as a matter of historical and etymological fact ' creation ', of course, *does* have a meaningfulness and legitimacy of employment in *certain contexts.* But it has notoriously undergone a series of transmutations and corruptions of meaning of which indeed the latest instance of degeneracy is to be found in the commentary accompanying the scientific theory we are examining. What is particularly noteworthy of this employment is the fact that the *last vestige* of meaning borrowed from its primary context of usage has been removed. This primary meaning of the term is to be found in the domain of human craftsmanship where it refers to the process of making some article of use such as a watch or a chair. What ' creation ' refers to here is at once a familiar and accessible fact of experience. Individual human beings, by virtue of some relatively distinctive use of skill and imagination are able to manipulate and transform already existent materials to yield a product whose structure and function can be appreciated by themselves or other members of the human community. When so located in this primary context of usage, the term ' creation ' allows of a literal analysis into such components as an agent or creator, materials used, methods of transformation or ' making ', and the finished product with its identifiable design or use. This literal meaning of the term has undergone, however, a variety of analogical extrapolations or truncations, which, while occasionally allowing innocent metaphors, have, instead of bringing fresh insight into another area, helped to breed confusion and support pseudo-explanatory devices. Thus what is in its primary usage a concept referring to a familiar fact of human experience, becomes under the pressure of uncontrolled analogical thought either a myth made to support ambitious metaphysical schemes, or, as it is transformed still further by theology, a cardinal mystery. The extension of the concept of creation beyond the domain of human craftsmanship to serve as a basis for cosmological speculation is the source of perhaps the most influential and persistent traditions of metaphysical thought. Its classic source is to be found in Plato. It is from Plato that metaphysical idealism and supernaturalism derive their inspiration in constructing a cosmology. Plato himself makes a clear and explicit appeal to human craftsmanship as the ' root metaphor ' employed in constructing his own theory of the universe. Plato's use of the imagery of human art in the myth of the *Timaeus* is guided by the profound conviction that such intelligibility as the world possesses is, at bottom, a purposive one, an adumbration of the Ideal Good. The cosmogony he pictures is conveyed in terms of a story not intended to be taken literally in its details. The Demiurge as ' creator ', the ' pre-existent, recalcitrant materials ' (the realm of Necessity and Chance) transformed by ' rational persuasion ' and purposive craftsmanship modelled on the Ideal and issuing in a world which exhibits a ' mixture ' of reason and necessity —all of these are not to be found in the ' creation ' of the cosmos in any literal way as is the case with ordinary craftsmanship. Conscious and deliberate myth here serves a philosophic conviction (itself the projection of a blind faith) in the designful character of the universe, the negation of all that the materialists and Sophists of Plato's day had proclaimed. Theology simply carried forward what Plato had here begun. What had been a conscious myth now became a literally intended mystery. The Creator not only cannot be located in the familiar world, He is no longer merely a symbol. His existence, literally claimed, is a ' transcendent ' one and basically an article of faith. Similarly the process of creation becomes a divine mystery, the most real of facts but shielded from human understanding.[1] Even human creation becomes, through an inverting rationalisation, no longer the primary and guiding image. Instead it is now *man's imitation* of the infinite divine capacity and as such accorded a secondary, derivative status.[2] Throughout there is the pervading faith, continuous with Plato's, in the purposive character of the universe, and all that it contains. As we turn to the present day, the manner in which we find creation appealed to in the steady-state theory is one which, in effect, carries this progressive mystification to its last stage. For all of the sustaining motives or analogical threads of comparison with art are gone. Scientific cosmology, of course, now not only makes no claims about the designful character of the universe ; it also stops short of making any reference to the Creator or the process of His making. It is not even claimed that these are

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[1] Hoyle, *New York Times Magazine*, loc. cit.

[1] cf. Augustine, *Confessions*, Book 11, §§ 4, 5

[2] cf. Maritain, J., *Art and Scholasticism*, pp. 123 ff.

mysteries whose existence is to be believed in even though not understood. All that it would retain is the fact that matter in an elemental form is created continuously. But if the Maker, the process of making, and the purpose are gone, *what is there left to the concept of creation ?* Doesn't the very description of the appearance or presence of matter as one which is due to creation lose *all* its significance ? Isn't it a case of its having lost not merely its primary meaning, but even its various attenuated analogical modifications as in metaphysics and theology ? If the *sole content* of the concept of creation is now simply that matter appears or is present, then far from this being a case of creation, it is at best, as previously suggested, a fact which invites scientific explanation.

6

We turn, finally, to an argument of a methodological kind offered by Bondi in justification of the use of the concept of creation. Here it is important to note that the types of argument employed on the one side by Hoyle and on the other by Bondi reflect two radically different philosophies of science at work, reflected in the way each proceeds to build up the theory, though the end results are essentially similar. Hoyle, who works out his proposals within the framework of the field equations of the general theory of relativity, introduces a modification in the expression of those equations which allows him the opportunity of developing a cosmological model different in crucial respects from those hitherto encompassed within the gamut of ' relativistic cosmologies '. The primary justification in his eyes for entertaining both the modifications in the equations and the resultant cosmology is the fact that it can be submitted to the test of prediction. We have already seen that this pragmatic appeal, correct and important as it is, in no way warrants the interpretation that if the theory proves successful, what has been confirmed is the claim that matter has been *created*. Meanwhile Bondi develops the theory along lines strongly reminiscent in some respects of the ' *a priori* deductive ' approach of Milne.[1] He would develop the cosmology not with the aid of the relativistic field equations but on the basis of what is regarded as a crucial principle, the *Perfect Cosmological Principle*. It is this principle which, it is claimed, warrants the introduction of the concept of creation.

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[1] cf. M. K. Munitz, ' Scientific Method in Cosmology ', *Phil. of Science*, 1952, **19**, 108

The Perfect Cosmological Principle may be formulated briefly as the statement that ' apart from local irregularities the universe presents the same aspect from any place at any time '.[1] Restriction to the case of spatial homogeneity, as is usually the case with the great variety of current cosmological models other than the steady-state one, gives what is called the ' narrow cosmological principle '. According to it, all positions in *space* are regarded as equivalent, from any of which a description of the universe can be made, since such descriptions will agree with one another.[2] Bondi would widen the principle to include a homegeneity or equivalence of times as well. The warrant for adopting the widened or ' perfect ' principle is two-fold, according to him. In the first place, unless the principle were adopted, there would be no justification for assuming the general validity of physical laws. Taking laws as generalisations, the argument maintains that all ordinary physical science rests upon the basic axiom of the ' unrestricted repeatability of all experiments '. The repetition of an experiment and the expectation that it will yield the same results as the law specifies, assumes that change of place and time in the performance of the experiment will have no effect upon the result.

> We see, therefore, that in all our physics we have presupposed a certain uniformity of space and time ; we have assumed that we live in a world that is homogeneous at least as far as the laws of nature are concerned. Hence the underlying axiom of our physics makes certain demands on the structure of the universe ; it requires a cosmological uniformity. [3]

Secondly, any attempt to apply the generalisations won on the basis of terrestrial experience to vaster regions of space and time and ultimately to the structure of the universe as a whole, requires some justification for extrapolating such generalisations. Here reliance is made on an argument originally due to Mach, and illustrated in connection with the dynamical fact of rotation, that there is a strong ' coupling ' between the outcome of terrestrial experiments and the distribution of distant matter, e.g. in the system of the fixed stars. It is maintained in a general way, similarly, that

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[1] Bondi, *Cosmology*, p. 12

[2] More accurately : ' All large-scale averages of quantities derived from astronomical observations (i.e. determinations of the mean density of space, average size of galaxies, ratio of condensed to uncondensed matter, etc.) would tend statistically to a similar value independent of the positions of the observer, as the range of the observation is increased ; provided only that the observations from different places are carried out at equivalent times.' Bondi, *Mon. Not. Roy. Astr. Soc.*, 1948, **108**, 253

[3] Bondi, *Cosmology*, pp. 11-12

> we can hence not contemplate a laboratory which is shielded to exclude all influence from the outside ; and for the same reason we cannot have any logical basis for choosing physical laws and constants and assigning to them an existence independent of the structure of the universe.[1]

Only some general assumption about the character of the universe at large will permit the use of laws and constants as holding without qualification throughout all regions of space and time. Here, moreover, instead of assuming that the laws and constants themselves undergo change, due to a general ' evolution ' of the universe, which would require—in order to make such a change or evolution itself meaningful and specifiable—the arbitrary stipulation that *some* laws or constants are indeed *invariable*, the proposal is made to avoid all such arbitrariness by rejecting all reference to an evolving or changing universe. This is accomplished by the Perfect Cosmological Principle which postulates that the universe is homogeneous and stationary in its large-scale appearance as well as in its physical laws.[2]

Use now is made of the cosmological principle in conjunction with the observed fact of local thermodynamic disequilibrium (the fact that more energy is found to exist in the form of matter than in that of radiation, as well as the fact that more energy is being radiated than is being absorbed by matter) to yield the conception of an expanding universe, a conception also supported by the observed recession of the nebulae. However, in order to satisfy the perfect cosmological principle, which requires a stationary (but not static) universe, together with expansion, there must be an abandonment of the principle of hydrodynamic continuity.

> By the perfect cosmological principle the average density of matter must not undergo a secular change. There is only one way in which a constant density can be compatible with a motion of expansion, and that is by the *continual creation of matter*. Only if the diminution of density due to the drift to infinity is counteracted by a constant replenishment of newly created matter can an expanding universe preserve an unchanging aspect.[3]

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[1] *Mon. Not. Roy. Astr. Soc.*, 1948, **108**, 253

[2] ibid., p. 254

[3] *Cosmology*, p. 143

The above argument involves two essential features of philosophic significance, one having to do with the logical status of the Perfect Cosmological Principle, the second with the 'deduction' allegedly made from it to the fact of creation.

The account given of the Perfect Cosmological Principle is in many ways strongly reminiscent of traditional discussions of the doctrine of the Uniformity of Nature offered as a solution to the problem of induction. As such, it suffers from precisely the same general difficulties that are already well recognised to hold for that more familiar formula, among them the fact that as a generalisation it has whatever weaknesses are alleged to belong to other generalisations. It cannot be known with certainty[1] if its truth-value is in any way dependent upon an appeal to experience. If experience, on the other hand, does not enter into its establishment, then one must claim for it self-evidence, but this notoriously fails as a criterion of truth. Interpreted in its most favourable light, the Perfect Cosmological Principle, like the Principle of the Uniformity of Nature, functions not as a factual statement at all, capable of serving as a premiss in an argument, but as a definition that functions as a criterion or rule of what in the language of science is to be regarded as a law. To be a law, the rule now asserts, is to be a statement which by its very *meaning* asserts a structural connection among a selected number of factors. This connection *could not be otherwise* than it is at various places or times without surrendering its own distinctive and individual nature. The law, consequently, *could not* change or be different ; at best we should employ *another* law and this in turn asserts a specific invariance or uniformity. So regarded, the Perfect Cosmological Principle is, to be sure, essential to science. However, it is not to be regarded as providing for other sciences a logical underpinning which is fathered upon cosmology. Cosmology shares such a principle equally with other sciences, since it is but a formal principle, a rule of the game, a defining characteristic of the techniques of representation which science employs regardless of its subject matter.

To say, moreover, that the Perfect Cosmological Principle is a formal principle means that it must surrender all power to serve as a premiss *in* an argument and the capacity to yield either by itself or in conjunction with other factual statements, results of a factual kind. Inasmuch as it is a rule governing the formulation and use of laws or theories, it cannot be regarded as *part* of a theory in cosmology. This, in effect, means that the criticisms made, for example, of other cosmological theories on its behalf (as supposedly the unique possession of the steady-state theory) are not warranted, and it also means that, in particular, the attempt to deduce from it, among other things, the existence of a continuous creation of matter is unjustified.

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[1] If the demand is not for certainty, there is no need for the Principle since any ordinary generalisation involves inevitably the hazard that it will not continue to hold in instances beyond those already examined. Its truth-value, now construed as its probability, is in no way altered by the appeal to the essentially vague principle of the Uniformity of Nature.

We must reject the claim that

> only in such a universe [the steady-state] . . . is there any basis for the assumption that the laws of physics are constant, and without such an assumption our knowledge, derived virtually at one instant of time must be quite inadequate for an interpretation of the universe and the dependence of its laws on its structure, and hence inadequate for any extrapolation into the future or the past.[1]

The steady-state theory is really in no better situation, methodologically speaking, than those theories which, for example, posit an ' evolution ' of the universe. For *any* theory, it is necessary to specify some relationship as invariant. If what are regarded as ' constants ' in one theory are regarded as ' variables ' in another, then in turn new constants must be set up to give the treatment some determinate form. Thus to be an item in an evolutionary process is to forfeit the status of being a law or constant. Only what expresses the structure of this process is entitled to this status. Now whether an evolutionary cosmology or a steady-state one is to be regarded as successful, cannot be settled by saying that for all theories but the steady-state one, the selection of laws is arbitrary. For in one sense *any* theory, by claiming certain relationships to hold and not others that are logically possible, is arbitrarily selective. Such selection must be justified now in the usual way by estimating the fruitfulness of its explanations and predictions.

Finally, no factual consequences such as are claimed to follow about the creation of matter can be drawn from the Perfect Cosmological Principle. To begin with, whether the universe is in a state of thermodynamic disequilibrium, or whether it is undergoing expansion, is something which we may claim to be the case or not, depending upon whether we take certain *arbitrarily selected* laws as holding in the interpretation of given observational data. These laws, such as the Doppler principle, or the various laws of thermodynamics, or atomic physics, that formulate the relations between matter and radiation, energy, entropy and the like, are, at best, useful devices, but by no means unique, i.e. without logically possible alternatives. The Perfect Cosmological Principle is not joined with these laws as another premiss to yield the result that the universe as a whole is in a state of thermodynamic disequilibrium or expansion. One uses the laws themselves that state the properties of thermodynamic disequilibrium or expansion in the interpretation of the data, but there is no requirement that they *must* be used. Finally, even were such interpretations to prove fruitful, it does not follow that the interpretation of the universe as being in a steady-state requires the creation of matter as a necessary condition. It would be sufficient for the purposes of the steady-state theory to propose that the average density of matter be constant, without presuming to offer in *that* theory any explanation for the appearance of matter so invoked. To provide such an explanation might be left for another theory of 'finer grain' that might be forthcoming, without in any way weakening or causing the abandonment of the steady-state theory. One would thereby eschew dogmatism and the surrender of the search for intelligibility that is involved in the appeal to 'creation'.

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[1] Bondi, *Mon. Not. Roy. Astr. Soc.*, 1948, **108** 254

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UTILITY THEORIES IN FIELD PHYSICS AND MATHEMATICAL ECONOMICS (I) *

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1 *Introduction*

The mathematical concept of Utility does not belong in economic doctrine alone; it has deep roots in natural sciences also. Mathematically oriented Utility considerations in science were actually developed long before a mathematical theory of Utility had been established in economics. Whenever a behaviouristic model construction for physical phenomena seemed adequate, Utility considerations were introduced into physical hypotheses. Economic considerations entered especially two chapters in physics: first the theory of equilibrium in classical mechanics, and second the theory of entropy in thermodynamics. As analogues or models for Utility in classical mechanics already had a firm foundation, pioneers of mathematical theories of Utility in economics (Walras and Jevons in 1871, Edgeworth in 1881, Pareto in 1896) were noticeably influenced by physical models drawn especially from classical mechanics.

Our historical survey will omit *pseudo-economic* interpretations in physics[1] and *pseudo-physical* interpretations in economics.[2] At various times, interpretations or analogies, which lacked mathematical basis, emerged. Utility theories in physics and mechanical analogies in economics will be of interest only according to their mathematical merits. Therefore, it seems opportune to outline briefly the mathematical attributes of Utility.

(1) Utility is a *function* with *several independent variables*. Its variations depend upon the *allocation* of stocks of commodities.

(2) Variations of a Utility-function are considered in *quantal* steps.

(3) Measurability of Utility on a numerical scale is not postulated; existence of comparable *utility-levels* is sufficient criterion.

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* Received 1. xii. 52.

[1] Examples: conservation of energy compared to barter; energy compared to the currency of nature; Carnot's formula explained as tax to be paid to nature etc.

[2] Examples: mechanistic theories of the physiocrats in the eighteenth century; technocracy in our day.

(4) Utility-levels can be comprehended in the form of *contour-maps of indifference*. Each level is characterised by an index.

(5) Utilities of commodities are *additive*. In the simplest hypothesis, additivity of Utilities is *scalar* in nature ; in more complex cases, summation of Utilities is subject to *vectorial* considerations.

(6) The Utility-function has a *maximum* or *maxima* with regard to given *constraints*.

(7) The entire theory of Utility can, *mutatis mutandis*, be translated into terms of *Disutility*.

2 *Utility theories in Classical Mechanics*

The theory of mechanical equilibrium dates back to ancient times. Mathematicians and physicists of antiquity knew and applied complicated conditions of equilibrium in numerous hypotheses. However, they did not possess a mathematical tool such as the concept of *function* and did not have insight into the variations of a function. Only after Descartes introduced functional considerations in natural science could economic reasoning enter the study of mechanical equilibria.

Utility considerations in classical mechanics began with the great mathematicians of the eighteenth century who were already adequately prepared to consider mechanical equilibrium problems in terms of variations. The following question, known as the *problem of many bodies*, was formulated : ‘ A system subject to an arbitrary set of forces will pass from its initial non-equilibrium position to final equilibrium through a sequence of intermediate states. On the basis of known initial data, can a mathematically perfect description of equilibrium and the sequence of intermediate states be *calculated ?* ’

The problem of many bodies represents the fundamental question of dynamics in the eighteenth century. Its relationship to theoretical economics is obvious. If a mathematical determination of the problem of many bodies exists, it would universally translate the behaviour of complex systems and offer a *model* for the solution of complex economic systems also.

Two distinct stages in the progress of ideas can be recognised. In the first phase, the *Principle of Least Action* was developed. The *principle of the minimum* itself can be traced to Hero of Alexandria (150–100 B.C. ?). In the seventeenth century Fermat established the principle of *least time* and applied it to propagation of light. The leading mathematicians of the eighteenth century wished to reformulate this principle in terms of Newtonian mechanics and to generalise it as the fundamental law of nature. They were inspired by a rational consideration. *Something* which nature wishes to bring to its extreme value, must *exist*. Thus Maupertuis accepted the principle of least action as a rational postulate and further specified it for the propagation of light. In the formulations crystallised by d'Alembert[1] the law of the minimum appeared in the form of the *Principle of Virtual Displacements*.[2] Every dynamical system is actually constrained by its internal structural liaisons in its prospective moves. Henceforth, the *ensemble* of driving forces in a dynamical system could do work only in this pre-set, constrained framework of *virtual* moves. But will these forces actually work ? By d'Alembert's principle, the driving forces will do work only if in their *ensemble* they can produce a positive net amount of work. Conversely, they will do no work if in each of the virtual moves, the summated virtual work is zero. With the aid of this principle, it becomes possible in numerous simple cases to test whether any proposed arbitrary situation does or does not represent an equilibrium. The proposed situation designates an equilibrium if *all* virtual displacements would lead to zero net amount of work. The test conditions for equilibrium can be written in the form of a system of differential equations to cover virtual work along all co-ordinates. It is understood that d'Alembert's model does *not* imply the flow of time. The mode of presentation is static and describes merely one position.

The Principle of Virtual Displacements has an *economic* meaning. If formulated in an anthropomorphic manner, as it often was, the principle means : Nature takes no step unless, in terms of her policy behaviour, it is *worthwhile* for her. The logic of virtual displacements is by no means a specific tool in the theory of mechanical equilibrium. The principle has more general significance ; its roots delve into the calculus of variations. In numerous hypotheses pertaining to extreme values the principle appears ; a particular value of a function is considered an extreme value if no displacement in the neighbouring region of hyperspace is conceivable. The principle applies also to other types of extreme value specified in calculus. The phenomenological meaning of the Principle of Virtual Displacements is : *self-sustaining tendency of the optimum.* Leibniz actually built his physics and metaphysics on the Principle of the Optimum.[1] A century after d'Alembert and Leibniz the Principle of Virtual Displacements was rediscovered, from the standpoint of economics, by economist-mathematicians and, also separately, by non-mathematicians, representatives of the Austrian School of marginal-utility doctrine.[2]

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[1] D'Alembert, *Traité dynamique*, 1743

[2] Before d'Alembert this principle had been stated by Jean Bernoulli and Leibniz.

The second phase of the dynamical doctrine based on least action was inaugurated by Joseph Louis Lagrange in his *Mécanique analytique* (1788). Lagrange recognised the universal validity of the principle of the minimum and its applicability to any problem in Dynamics. He sought a new model based on a *central characteristic function.* Is such a central entity derivable which will vary *monotonically* through a series of intermediate states until equilibrium is reached, and which will attain its extreme value just in the state of final equilibrium of the system ? To follow the variations of such a central and characteristic entity through all intermediate states until equilibrium was reached a new calculus was needed. Lagrange developed the calculus of variations. Without this tool, which was lacking in the dynamics of d'Alembert and Leibniz, only *conditions of equilibrium* could be stated. Lagrange wished to solve the problem of many moving bodies in the most universal sense, i.e. to determine *every* displacement of *every* particle from the initial state of a system to its final equilibrium. He conceived the mathematical model of *Field of Force* into which he incorporated the Principle of Virtual Displacements and Newton's laws of gravitation.

The Field-model was developed by Lagrange in the form of a certain configuration in hyperspace. Lagrange assumed that a centre of forces (e.g. a gravitational centre) assigns certain physical properties to the particular points of the surrounding space. By resorting to a four-dimensional continuum, it would be possible to associate each bundle of space co-ordinates with some kind of a ' field-characteristic ', namely, in the form of an added fourth co-ordinate. The resulting four-dimensional configuration would then present comprehensive information regarding the physical properties ascribed to space and thus would represent the field of force. Does a similar configuration actually exist ? To build the fourth co-ordinate in connection with gravitation, Lagrange suggested introduction of the entity ' potential ' (= work done by the force-centre when bringing a unit mass from an infinite distance to the particular point of space). ‘ Existence ’ of the potential field can be best envisaged in the form of its projections from four dimensions to geometrical space. In a similar projection, points in space associated with equal potentials will be arranged in a distinct geometrical order, i.e. in the form of non-intersecting and ordered layers of contour-surfaces. The gradients of the four-dimensional configuration (expressing directions and magnitudes of the maximum change of the potential) can also be projected. Their projections will denote field-intensity (= force exerted on the unit-mass) ; the *ensemble* of the projected gradients will delineate a system of lines of force.

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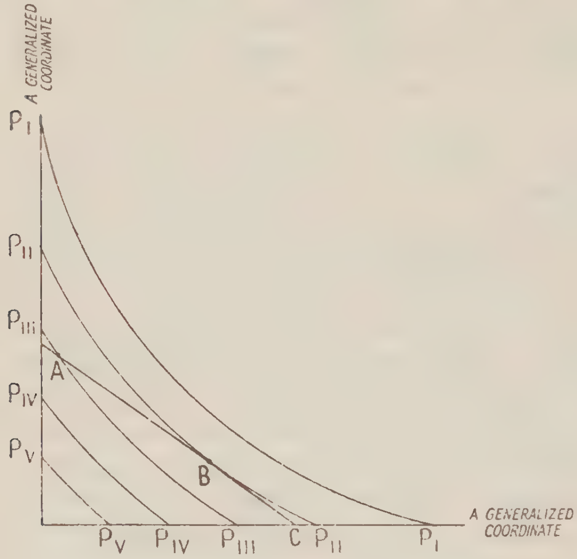
[1] Leibniz, *Essai de théodicée sur la bonté de Dieu*, 1710

[2] Carl Menger, *Grundsätze der Volkswirtschaftslehre*, Wien, 1871 (English edition 1934) ; Friedrich Wieser, *Der natürliche Werth*, 1889 (English edition 1938).

The Field-model proves as a powerful tool in analytical mechanics for the description of particle-motions subject to a central force. Each change in the position of an *ensemble* of particles now can be thought of in terms of a corresponding set of paths between initial and terminal points. The central problem of analytical mechanics is calculation of the paths in the reference frame of a potential field. What is actually the course of a single particle from an arbitrary initial point of the field and where will its terminal point be located ? Vector analysis verifies that even in the case of the simplest (‘ conservative ’) types of fields, two additional sources of vectors, besides the forces of the field, will also intervene. First, the implication of constraints has to be appropriately treated. Namely, generally speaking, a field of force is not expected to operate freely, i.e. in an unobstructed way. A constraint can be formalised in the form of a constraining surface comprehending certain additional bundles of vectors imposed on the affected regions of the field. For the purposes of a unified computation of forces and constraints, Lagrange devised the technique of multipliers. Second, vector analysis will have also to include the vectors brought forth by the particle itself, by virtue of its kinetic energy. To treat this particular issue in the reference frame of a field, Lagrange introduced the *L*-function.

On the basis of the above model the behaviour of a particle can be perfectly understood. The actual configurations of all vectors along all the infinitesimal path-elements will account for all kinematic properties of particle-motions. Lagrange derived differential equations for the solution of each separate problem. His fundamental equation formulates the general law of motion which describes the behaviour of a particle in a field of force. Lagrange’s second fundamental accomplishment pertains to determination of the static equilibrium of the particle. Disregarding the particle's own kinetic energy, the forces of the field would impel the particle down to the lowest attainable potential contour compatible with the constraints, viz. along the shortest paths on the surface of constraints. In the point of static equilibrium, by definition, the surface of constraint becomes tangential to a potential surface (see Fig.). Restated in another way, in the point of static equilibrium, the gradient of the potential (= force), becomes perpendicular to the surface of constraint. On the mathematical side, static equilibrium of a particle is determined by the minimum value of the potential function under the given constraints. Its computation is based on a certain system of differential equations derived from the potential function and supplemented by considerations translating the constraints.



A Field of Forces under Linear Constraints

Point A represents an arbitrary initial state at potential P_{III}
Point B represents static equilibrium at potential P_{II}
The family of level curves P_V, P_{IV}, P_{III}, P_{II}, P_I, etc., represents levels of potential in decreasing order
Line A B C, a line of constraint in projection, is tangential to potential curve P_{II} at point B. All gradients of potential surface (force-components) are perpendicular to the line of constraints at point B
The potential difference $P_{III} - P_{II}$ represents *work* done in the process

Lagrange's final accomplishment pertains to the description of the dynamical behaviour of the particle. It is understood that the particle, owing to its own kinetic energy, will not rest in the point of static equilibrium, but will remain in motion within a certain region of the field. Thus the Lagrangean concept of equilibrium allows, besides static equilibrium, the consideration of dynamical stability. A system is termed stable if constrained to a particular region of the field.

The Lagrangean system was further developed by Laplace and applied with success in his *Traité de mécanique céleste* (1799–1825) to particular problems in celestial mechanics. Lagrange and Laplace represent the eighteenth century's optimistic conception of ideally perfect mechanics. Nature's laws are completely determined. Everything is calculable and predictable ; ' savoir c'est prévoir '.

Lagrange's and Laplace's attention was focused on the problems of the gravitational field that concern celestial mechanics. The use of their generalised co-ordinates was further developed by Hamilton in 1834. It was understood that the Lagrangean particles do not interact ; all particles hitherto were assumed to perform their motions independently. Such idealisation, permissible in celestial mechanics, is obviously unrealistic when describing terrestrial systems. Some further generalisation of the Principle of Least Action was needed to cover not only the motion of single particles but also universal interaction among particles. According to Hamilton's Principle, the single particle tends to build over its actual path a certain time integral which has a ' stationary ' value (in most problems encountered as a minimum) when compared with this time integral over all corresponding ' varied ' paths. This principle made possible a restatement of the equation of motion in its most general (' canonical ') form and explained the behaviour of systems of particles, i.e. any kind of dynamical system involving mechanical forces. Hamilton developed also the new vector calculus needed for treatment of the arising dynamical problems, i.e. the calculus of quaternions.

The next step, which was undertaken by Gauss in 1839, was to apply the Field-model to electrostatic phenomena. Clausius in 1850 initiated the Field-analogy treatment of thermodynamical processes and formulated the corresponding Field-function in terms of entropy. A new chapter of Field Physics had its start and developed independently along its own ramifications. It is most instructive to realise that comprehensive tables of valid analogies can be established between the key-concepts of the theories of potential and thermodynamical entropy to demonstrate the common origin of the concepts in classical Field Physics.

Two further steps helped the progress of Field Physics. To interpret the genesis and properties of *waves* in the Field frame of reference, Maxwell[1] established the model of electromagnetic Field and stated his field equations in 1873. Practically all known types of physical phenomena were already included in Field-models ; however, all these different Fields were formulated separately. The following step in Field Physics already envisaged the coming unification of Fields. A thorough remodelling of the concept of ' configuration of co-ordinates ' became imperative. From the Principle of Relativity stated by Poincaré in 1904 and by Einstein in 1905 it was understood that a substantial part of our experiences both in the sectors of cosmic events and of the atom, are merely relativistic, i.e. dependent on the observer's position. To comprehend similar event-histories, a new type of Field-model had to be devised which covered relativity of time and space. From Lagrange to Maxwell, Field-models included time as a universal and absolute reference-frame of flowing events. In relativistic Fields, time constitutes a fourth co-ordinate associated with each particle to specify its position in the unified space-time continuum. Besides the geometrical properties of the gravitational field, the Newtonian laws of dynamics had also to be appropriately reconsidered to be fitted into the relativistic framework. After incorporating into the Field-model all the necessary ' relativistic corrections ' (including equivalence of matter and energy and their joint conservation), Einstein derived the general law of motion in the relativistic field as a new variant of the Principle of Least Action. The world-line, which is the event-history of a particle, follows the shortest possible paths (' geodesics ') determined by the curvatures bent upon the continuum by gravitational interaction. Subsequent formulations in relativistic mechanics expressed the Principle of Minimum Action in a new light. Hilbert stated that gravitation brings the total curvature of space-time continuum to a minimum. According to Whittaker gravitation represents a continued effort for the universe to straighten out. *The Unified Field Theory* suggested by Einstein is destined to represent the final generalisation of the Principle of Minimum with respect to all phenomena, including mechanics and electromagnetism.

The essence of Field Theory can be summarised as follows :

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[1] Maxwell, *Electricity and Magnetism*, 1873

(1) All event configurations happen in a topologically completely-ordered continuum.

(2) The sequence of actual events produces some type of extreme value under the constraint of a certain law of conservation.

(3) Dynamic interferences with a system can be described as curvatures bent upon the continuum.

3 *Mechanical Analogies in Economics*

The classical concept of ' configuration of co-ordinates ' proved a guide in mathematical economics. After Cournot's[1] initiative (1838) in formulating the equilibrium of a single market, and Gossen's[2] incentive (1854) to specify in a geometrical form a single individual's economic behaviour, Léon Walras[3] was the first (1871) to recognise the feasibility of dynamic economic analysis in general. He attacked dynamic analysis of economic systems at three different levels. The first level implied solution of an individual's budgeting with a given income at a given price-level. This frame of reference merely denoted the solution of a particular type of equilibrium, i.e. partial equilibrium. The Marginal Utility Theorem stated by Walras with the methods of the calculus was only the starting point.

Walras was first to introduce the conception of a *general economic equilibrium* to resolve the global result of the *ensemble* of partial equilibria. This was his second-level of approach. It was born under the auspices of the eighteenth century's *Mécanique rationelle*. Walras aimed to establish a perfect knowledge of all markets considered *simultaneously* and formulated the model of *Économique rationelle*.[4] Perfect knowledge of all markets could be attained through a system of simultaneous equations representing constraining conditions for a steady state which can be identified as economic equilibrium. The problem of such equilibrium is considered as mathematically solved if the number of available equations equals the numbers of unknown (the unknowns being the quantities exchanged and their relative prices).

This second approach of Walras reflects the Principle of Virtual

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[1] A. A. Cournot, *Recherches sur les principes mathématiques de la théorie des richesses*, Paris 1838 (Eng. trans., New York, 1897)

[2] H. H. Gossen, *Entwicklung der Gesätze des menschlichen Verkehrs und der daraus fliessenden Regeln für menschliches Handeln*, 1853

[3] Léon Walras, *Eléments d'économie politique pure*, 1871

[4] Terminology introduced by François Divisia, *Économique rationelle*, Paris, 1928

Displacements borrowed from d'Alembert's Dynamics, although Walrus did not formalise the analogy as such. The *ensemble* of forces implied in a dynamical system does not produce work unless they can generate a net amount of work. Now, external economic forces can be identified with causes which modify existing flows of commodities supplied and demanded in the steady state. Without interference of such external causes, flows of goods would remain steady like the flow of a stream. The internal economic forces of supply and demand will become immediately activated if, for any reason, discrepancies between supplies and demands arise. Shifts will then materialise in the sectors both of supply and demand, and the system will attain its new equilibrium position ; supplies and demands become equalised once again. By d'Alembertian logic, economic equilibrium can be identified with a situation in which, under the given constraints of the free competitive economic system, no finite shift in any of the sectors of supply and demand can materialise. Walras formalised the constraints in the form of an equation translating the system's virtual variations ; in a set of additional equations he comprehended all possible configurations under the driving forces of supply and demand. Finally he showed that in all specified types of general equilibrium models, the number of unknowns is equal to the number of functional liaisons. It is understood, however, that time-flow was not included among the co-ordinates ; the Walras-d'Alembertian model is timeless.

To the problem of general equilibrium Walras formulated a second solution. He established the concept of a central entity which is expected to reach its maximum in the equilibrium position of the markets. He stated his Theorem of Maximum Satisfaction. Economic[1] equilibrium secures the greatest possible satisfaction for society within the limits of constraints imposed on the system by free competition. Walras' system of simultaneous equations, interpreted as the determinant of the extreme value of a central characteristic function, undoubtedly pointed toward the Lagrangean Field Analogy. Field-interpretation of economic equilibrium represents the third level in Walras' dynamic analysis.

Does a more specific structural analogy between Lagrange's mechanics and Walrasian economics exist ? The author of this study[2] advocated the possibility of such analogy in 1932. Luigi Amoroso[1] in 1948 stated the possibility of specifying formal parallelisms between the different groups of Lagrange's and Walras' equations. Analogies are tempting and therefore must be judged with circumspection.

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[1] Walras, *Eléments*, pp. 133, 286

[2] A. Pikler, 'Die dynamischen Geldtheorien und die mechanischen Analogien', *Ungarische volkswirtschaftliche Rundschau*, 1933, Nos. 1-2

In a general and merely formal sense the mathematical analogy is undeniable. In both theories the problems focus on *variations of a central characteristic function* which are expected to be brought to their *extreme value* under their respective *constraints*. A group of liaisons which establish the functional variations of a dynamical system exists in the Lagrangean Field-Theory. Particular equations describe constraints which are manifest in the form of virtual variations. In the state of equilibrium, all functional variations are constrained to the minimum level of potential. Similarly, the Walrasian system implies a group of equations formulating functional economic variations; particular liaisons are stated to specify the virtual variations of the economic system. In the state of equilibrium all functional variations are constrained to the maximum level of ' collective satisfaction '. This analogy is confined to static equilibrium.

Walras obviously recognised the intricacies involved in a presentation of the Analogy in more specific terms. At first sight, a difficulty appears in the mathematical sector. Lagrange's theory was built on a *minimised* function, whereas Walras felt bound to formulate a superfunction of the market in terms of an entity to be maximised. Were an inverse approach to be chosen, the entire economic model ought to be recast in terms of an artifact, i.e. the ' function of social *dis*utility ' ; clarity of the model would be certainly overshadowed. Apart from such formal implications, the meritory aspect had to be really clarified, i.e. the ' existence ' of the superfunction in the mathematical sense as an outgrowth of functional economic variations. Hereafter, phenomenological difficulties would accumulate if a specification of the parallelisms concerning the variables were attempted. Namely, in Lagrange's theory ' configuration of co-ordinates ' cover spatial distribution of particles ; differentials mean infinitesimal displacements in space. In turn, ' configurations of co-ordinates ' designate in the Walrasian model quantities of commodities supplied and demanded during a time unit, i.e. commodity flows ; differentials represent their increments. Moreover, Lagrange's dynamics deals with allocation of particles in three dimensional Euclidean space, whereas no geometrical space in the Walras model is involved.[1] Also time was treated in a different manner in the two models. In Lagrange's dynamics, time was included in the formulation of constraints ; the kind of time implied was Newtonian, i.e. a general reference-frame of flow of events, universal in its validity and independent from the observer's position. None of Walras' three models included the issue of time-flow among the co-ordinates ; therefore no analogue to Langrange's equation of motion to specify temporal history of configurations could be obtained. Moreover, all *Field-characteristics* in *Mécanique rationelle* were drawn from issues in Newtonian physics (force, work, etc.). Field-characteristics in *Économique rationelle* must be based on specific implications of *human desires* and *social constraints.*

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[1] Luigi Amoroso, ' Théorie mathématique de l'équilibre économique : équations générales de la dynamique ', *Econometrica*, 1950, **18**, 64

Without even envisaging the possibility of analogies, the field-formulation was used by Walras with extreme caution. The Theorem of Collective Satisfaction was stated only as a corollary. Because of possible considerations regarding the concept of collective satisfaction and its measurability, Walras refrained from building his theories on a foundation of a Field-analogue kind. His conception, which was primarily based on causation through sets of forces rather than on the collective behaviour of the system, pointed toward the pre-Lagrangean approach in dynamics. The first two levels in his dynamic analysis, i.e. the solution of individual budgets and of general equilibrium by means of systems of equations, reflect a d'Alembertian approach. A nucleus of the Walras-Lagrangean analogy can also be traced.

(*To be concluded*)

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[1] The *ensemble* of all markets may be thought of as a 'configurational economic space' which encompasses the aggregates of k different types of commodities distributed among n individuals, i.e. a grand total of $n \times k$ 'commodity-stacks'.

THEORY OF INTEGRATIVE LEVELS*

James K. Feibleman

From the study of the relations between the fields investigated by physics, chemistry, biology, psychology and anthropology, some knowledge of a structure is gradually emerging.

The theory of the integrative levels has been growing in recent decades. Men like Bertalanffy,[1] Needham, Novikoff[2] and others have concerned themselves with it. Much remains to be done. Bertalanffy[3] has envisaged a sort of super-science which shall have as its subject-matter the relations between the sciences. The philosophy of science may yet be the source for the development of an empirical field itself consisting of the integrative levels, a sort of meta-empirical field, with its own entities and processes and laws.

1 *Some Laws of the Levels*

In this and the following section are set forth some of the uniformities which are found among the integrative levels. A few only are given ; others surely exist and can be added as they are discovered.

1. *Each level organises the level or levels below it plus one emergent quality*. Thus the integrative levels are cumulative upward. This proposition implies that everything has at least the physical properties and has led to the position of supreme importance of the physical world in science and philosophy. Chemical elements have mass, density and dimensions, and so do biological organisms ; it is possible to weigh aluminium or to measure the height of a horse. But that everything has at least physical properties does not mean that anything

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* Received 30. iv. 53. The author wishes to acknowledge the valuable assistance of Professor William L. Duren, Jr., Head of the Department of Mathematics in Tulane University, who is not necessarily in agreement with all of the views expressed here, however.

[1] Ludwig von Bertalanffy and J. H. Woodger, *Modern Theories of Development*, Oxford, 1933

[2] Alex B. Novikoff, ' The Concept of Integrative Levels and Biology ', *Science*, 1945, **101**, 209–215

[3] Ludwig von Bertalanffy, ' An Outline of General System Theory ', *The British Journal for the Philosophy of Science*, 1950, **1**, 134

except physical objects has only physical properties. Ionic exchange takes place at the chemical level, and organisms are self-directive ; for instance, when hard water is passed through a zeolite, the calcium ions are replaced by sodium ions ; and the tissue of horses when slightly injured will heal by itself.

2. *Complexity of the levels increases upward.* This law is a corollary of the first. For if the levels are cumulative upward, each must be more complex than the one before it. The structure of the atom is an organisation of electrons, protons and neutrons. The structure of the molecule is an organisation of atoms. The structure of the cell is an organisation of molecules, and so on. Thus the entire structure of the integrative levels rests on the physical as on a foundation and rises above it in a series of increasing complexity based on complications of the lowest. We have seen this in the structures themselves, but it is not so evident in the emergent qualities. At the higher levels the emergence of qualities marks the degree of complexity of the conditions which prevail at that level. It would be theoretically possible to assign numbers to the levels to indicate their degree of complexity by counting emergent qualities, though this has not yet been done. As yet we are not sure of the qualities. It is the qualities, then, which lend to the integrative levels their relative autonomy. For every quality is as much a quality as any other, and none which is the product of higher grades of complexity is any more qualitative than those which exist at the lower grades of relative simplicity. Qualities at higher levels are somewhat more elusive yet none the less real, none the less qualities *qua* qualities.

3. *In any organisation the higher level depends upon the lower.* That is to say, in an object which extends over more than one level, which it must if it exists at any level above the physical, then the higher level depends for its continuance upon the lower. A culture will not last if its cities are destroyed ; a chemical compound will not continue if its bonds are broken ; yet both cultures and chemical compounds will leave behind them physical elements ; rubble in the case of the cities, and single elements in the case of the compound. The physical properties are more enduring than the constructions raised over them. Destroy the physical properties in any object and the entire object goes, but the reverse is not true. A man run over by a street-grading machine is no longer a man, no longer even an organism but merely a collection of decaying cells, eventually to be reduced to still lower levels, to carbon compounds.

4. *In any organisation, the lower level is directed by the higher.* That is to say, in an object which extends over more than one level the higher level furnishes the direction of the lower. An army may travel on its stomach but it is its brain that tells it where to travel. Institutions may have physical perquisites, such as buildings and tools, but they take social aims. Just as the mechanism of an organisation is furnished by its lower levels, so its purpose is the product of its higher levels. Purpose in science has been in bad repute for some centuries, and has been replaced by mechanism, chiefly because of its theological reading. Science quite properly avoids all transcendental notions, being committed to what it can learn from the combination of logic with fact, and so has read all teleology as outside its province. But purpose, conceived as vectors built into organisations and discoverable there, has a legitimate function in science. It would be difficult indeed to understand the somatic organism without the aim of adaptation presupposed. One might even add that nobody can understand the mechanism of any organisation without some implicit preliminary assumption of an hypothesis with regard to its purpose. It would be quite possible in perfect innocence to take an automobile apart. But it would not be possible to put it back together without knowing what it was intended to do.

5. *For an organisation at any given level, its mechanism lies at the level below and its purpose at the level above.* This law states that for the analysis of any organisation three levels are required : its own, the one below and the one above. To analyse a mechanism we drop down one level. This is obvious when we consider that analysis moves from the whole to its parts. For we should not expect the parts to be on the same level of analysis as the whole of which they are the parts.

Similarly, to find the purpose of any organisation we would move up one level, for then we are considering the organisation as itself a part of some higher and more complex organisation. We could perhaps study the kidney and its functions without reference to its constituent tissues and without reference to the somatic organism in which it plays a certain rôle or rôles, but we would not get very far without the introduction into our considerations of both adjacent levels.

6. *A disturbance introduced into an organisation at any one level reverberates at all the levels it covers.* It is now well known in psychiatry that mental events can occasion physical difficulties and *vice-versa.* Anxiety may give rise to diarrhoea, or conversely traumasthenia may occur when nervous exhaustion follows physical injury. Disease may

occasion loss of weight as well as depression. We are assuming here, of course, the levels as they occur in highly integrated organisations. In less integrated organisations the reverberation would be neither as extensive nor as severe.

7. *The time required for a change in organisation shortens as we ascend the levels.* The evolution of stars requires a longer time than the evolution of biological species, and the evolution of biological species requires a longer time than change in human cultures. Atoms and molecules have been around longer than cells, cells longer than the more complex organisms and these longer than human cultures. The chemical elements are reputed to be something over 3 billion years old, the cultures merely several thousand. The life-span of the genus, horse, has been calculated at about 5½ million years, while 20 million years is the estimated duration of the species of Triassic ammonites.[1] Evidently the more complex the organisation the more unstable, so that advances are not achieved without a price and advanced positions are not easily held. Hence duration is a function of the integrative levels.

8. *The higher the level, the smaller its population of instances.* The organisation at each level upward is a complication of the organisations of lower levels. Therefore, as we should expect, there are fewer molecules than atoms, fewer cells than molecules, fewer organisms than cells and fewer cultures than organisms. There are more electrons in the universe than anything else, but cultures are rare : Toynbee professes to find no more than twenty-one instances of the species. From the point of view of population the integrative levels form a pyramid.

9. *It is impossible to reduce the higher level to the lower.* Each level has its own characteristic structure and emergent quality. Hence, to reduce a higher to a lower level means to lose the quality and the structure of the higher level. A living organism is not merely a collection of tissues and cells and organs, although it contains these. In the organisations which exist at each of the integrative levels no whole is merely the sum of its parts but each contains the higher organisation of the whole itself. The attempt to reduce the higher to the lower level comes about as a result of the familiarity with analysis in scientific method. Science is seeking primarily the mechanism of the organisations into which it inquires, and this, as we have noted,

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[1] F. E. Zeuner, 'Time in Evolution', *The Smithsonian Institution Report for 1949*, Washington, 1950, pp. 247 ff.

requires dropping down one level : parts do lie at a level below the whole. However, this seems to have led some investigators to suppose that the parts are what is real about the whole. The truth seems to be that the whole and its parts are equally real ; and indeed the question of which is parts and which is whole becomes merely a question of the level chosen for analysis, for there is some level at which every organisation is a part of some whole and another level at which it is a whole to its parts.

To suppose that the higher level can be reduced to the lower is to commit the fallacy of reduction. The failure to understand this fallacy has led to errors in interpreting scientific method, on the fond assumption that since the wholes are found at the level of common sense and the analytical parts are revealed only in laboratory procedures, the former must be illusive appearances while the latter are the realities. But the table is not merely a collection of whirling electrons, it is also a table, a real culture object as well as a real piece of wood.

10. *An organisation at any level is a distortion of the level below.* Higher organisations disturb the smoothed out distribution of lower organisations. Matter, it has been argued, consists in crinkles in space-time. And organisms have been thought of as distortions in the crystallographical field.

11. *Events at any given level affect organisations at other levels.* This is the same phenomenon as the one discussed in Law 6 above, only this time seen from the outside instead of the inside. The failure of the corn crop is supposed to have destroyed the Mayan culture of Yucatan and Northern Guatemala. Wars at the culture level, on the other hand, may cause the death of many organisms and of many more cells.

12. *Whatever is affected as an organisation has some effect as an organisation.* Examples of characteristic behaviour are : cause-and-effect at the physical level, combination-rearrangement at the chemical level, sensitivity-reactivity at the biological level, stimulus-response at the psychological level and contact-adaptation (as this takes place between cultures) at the cultural level. Every organisation at whatever level it exists has some sensitivity manifested as irritability, and makes its responses in kind.

2 *Rules of Explanation*

1. *The reference of any organisation must be at the lowest level which will provide sufficient explanation.* We are forbidden to call in higher levels for phenomena which are entirely explicable at the lower.

Patients who give allergic reactions to eggs need not be treated for neurosis even though their symptoms may have indicated either. We are obliged by the principle of economy to account for as much as possible by explanations which are as simple as possible, and this means going no higher in the integrative levels than our material requires us to do. Stars do not have souls, but on the other hand men are not merely carbon compounds. If you can entirely account for a compound at the chemical level there is no need to go to the biological for an explanation.

2. *The reference of any organisation must be to the highest level which its explanation requires.* This rule is the converse of the previous one. We must not go any higher in the integrative levels than necessary to provide sufficient explanation, but we must go as high as is necessary. Complex phenomena probably call for higher types of explanation. The lowest levels are complex enough to make explanation a difficult affair, but over-simplification does not explain : it explains away. An organisation, in short, cannot be explained without bringing into the explanation elements which belong to its highest level.

3. *An organisation belongs to its highest level.* This is a very important rule of explanation. All organisations except the physical belong to more than one level but each must be considered as belonging in some peculiar way to its highest level. Chairs like men are not mere physical objects, though they are that too; they are culture objects. Human beings are not mere reactive mechanisms; they are also responsible members of society. An atom is not merely a collection of electrons and protons ; it is also an atom.

4. *Every organisation must be explained finally on its own level.* A broken leg cannot be accounted for on social or psychological grounds nor on physical grounds either for that matter. We need to know the tensile strength of the Fibula or Tibia, in this case the physical properties of biological material.

5. *No organisation can be explained entirely in terms of a lower or higher level.* This is obviously a corollary of the last principle. Mechanism and vitalism in biology are attempts to explain organisms in terms of lower and higher levels respectively.

3 *Extended Theory of the Levels*

We have been talking about the integrative levels of the scientific fields as if only some five were involved. This was necessary in order to see clearly some of the relations. But the situation is more complex than that. For each level is the name for a very considerable group of sub-levels. As our investigations continue, the complexity of the levels is increasingly revealed. The picture will therefore have to undergo continual improvement.

Our old integrative levels are now generic names for groups of integrative sub-levels, and there is reason to suppose that further inquiry will mean still further sub-divisions. The sub-divisions, it may be remarked parenthetically, are thickest where investigation has been the most successful.

There is a level below physics. This is geometry, conceived as the level of mere extension, of space relations, and within that region the existence of proximate natural sets of objects. Further distinctions are already beginning to appear. It may be, for instance, that the chemistry of supermolecular structure consists in another entire set of fields, generically crystallography and including the sub-divisions of solid crystals and above that of liquid crystals or paracrystallography. The crystal field shades up from chemical synthesis, petrology or mineralogical chemistry through crystallography proper to paracrystallography or liquid crystals.

If we add crystallography to our series, we complicate the situation somewhat. For the crystal level builds not in one way but in two. The crystal, or rather the paracrystal, level builds to the biological in one direction, but in another it does not increase the complexity but in number and combination, giving rise in this direction to the astronomical level. Astronomical bodies seem to be vast accumulations of molecules in cloud gases and stars and of crystals in planets. This, so far as our limited knowledge goes, would appear to be a dead end ; while the biological continues to build into the psychological and the cultural. These considerations will compel us to revise our simple linear integrative series of levels.

4 *Theory of Branching*

Thus far we have been regarding the integrative levels as a simple linear series of complexities increasing upward in the scale. But once again we must complicate the picture if we are to be faithful to the facts of the situation. Since Comte it has been customary to refer to the integrative levels on analogy to arboreal branching, a method of further dividing and sub-dividing. By the branching of the integrative levels is meant that certain levels build up to two or more fields which may either sub-divide further or come to a more or less abrupt end of level-building.

The branching of the integrative levels raises more questions than it settles. For instance, does the run of the empirical fields as they are now constituted exhaust the possibilities, or is there more to come ? The picture as we have it seems unfinished because of the dead ends and also because of the asymmetry of the whole. Are there infra-geometrical and supra-cultural levels which have thus far escaped our sensibilities and our instruments ? We have as yet no way of knowing. We do not even know whether the limitations are those of existence itself, of the integrative levels of the scientific fields, or of our own poor limited sensibilities. All that we see at the present time is an asymmetrical picture of a process which is unfinished, the sinistral dead ends and the dexterous, presumably continuing functions. Such, however, are the fields which the sciences study, and we can deal with them only as they are.

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NOTES AND COMMENTS

Notes on a Remark by Lord Russell[1]

RUSSELL[2] asserts that the probability that a mathematical deduction is free of error after a given number of revisions can be calculated. It is intended in this note to express this assertion quantitatively.

If, as a result of an error in a mathematical deduction, a deductive step taken is not only invalid but also leads to a proposition which cannot be validly deduced, then (provided no further invalid steps are taken) either all subsequent propositions are non-deducible, or the (unbroken) sequence of non-deducible propositions comes to an end before the deduction is ended, in which case the following propositions must be *all* deducible, including the final conclusion (except if one of the non-deducible propositions is used more than once as a premise).

We consider here only the extreme cases :

I. where the (unbroken) sequence is of unit length so that no subsequent step is invalidated ;

II. where all subsequent propositions are non-deducible.

§1 The assumption that the errors committed in any steps are independently corrigible is rarely true in mathematical work ; nevertheless, the formulae obtained on this assumption might serve as a first approximation. For other types of problems, e.g. that of mistakes in copying, the formulae obtained might well be strictly applicable.

Let a deduction consist of a finite number N of steps, where $N > 1$, and let the probability of a step leading to an invalid conclusion (which may be called, simply, an error) be α, where $0 < \alpha \leqslant 1$. Denote by β_n the probability that any given error remains undetected at the nth revision, where $0 < \beta_n < 1$.

Then after x revisions the probability of having no undetected errors is

$$\left(1 - \alpha \prod_{1}^{x} \beta_n\right)^N \tag{1}$$

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[1] Thanks are due to the referee for a correct reformulation of the second paragraph, and to Associate-Professor M. H. Belz and Dr G. S. Watson for helpful discussions.

[2] Bertrand Russell, *Human Knowledge : its Scope and Limits*, London, 1948, p. 400

and the expected number of undetected errors is

$$N\alpha \prod_{1}^{x} \beta_n. \tag{2}$$

With regard to the β_n's two alternative assumptions will be considered.

§1.1 Assume first that the probability of detecting errors is the same in each revision so that

$$\beta_n = \text{constant} = \beta' \text{ say.}$$

From (1) it follows that the number of revisions, x say, to secure with probability p that no errors remain undetected is

$$x = \frac{\log(1 - p^{1/N}) - \log\alpha}{\log\beta'}. \tag{3}$$

As x is finite for any $p < 1$, it follows that certainty of freedom from error can be approximated as closely as desired by a finite number of revisions.

§1.2 On psychological grounds it is perhaps more realistic to assume that an error once committed has a greater probability of escaping detection than of being committed in the first place. Analogously, the probability of overlooking errors increases from one revision to the next. We thus have

$$0 < \alpha < \beta_1 < \beta_2 < \ldots < \beta_n < \beta_{n+1} < \ldots < 1. \tag{4}$$

There are two possibilities to be considered :

§1.2*a* β_n increases so slowly with n that $\sum_{1}^{\infty}(1 - \beta_n)$ diverges.

In this case $\prod_{1}^{\infty}\beta_n = 0$ so that, as in I. 1, certainty of freedom from error can be approximated as closely as desired by a finite number of revisions.

§1.2*b* β_n increases so fast that $\prod_{1}^{\infty}(1 - \beta_n)$ converges. In this case $\prod_{1}^{\infty}\beta_n = \beta$, say, where β is a fixed number between 0 and 1. From (1) and (2) it follows that no finite number of revisions can reduce the expected number of undetected errors to its lower bound $N\alpha\beta$, and that a finite number of revisions can increase the probability that no errors are left undetected to p if, and only if, $p < (1 - \alpha\beta)^N$.

§2 When all propositions subsequent to the first non-valid step are non-deducible, we may proceed as follows.

Revisions must be repeated until the first error is detected; let this set of revisions be called a 'successful set'. After the completion of the successful set *all* steps subsequent to the detected (and corrected) error must be reworked; in this process new errors may be committed, the first of which must again be detected by another successful set of revisions, and so on until the whole deduction is free of error. The number of successful sets needed to achieve this freedom is the same as the number of subsets into which the 'first errors' (i.e. the first error committed in the original working and the first error committed in each reworking) break up the whole set of N steps. We denote the number of steps in any subset by h; the expected number of steps in each of these subsets is $1/\alpha$.[1] The expected number of successful sets, H say, needed to free the deduction from errors is the expected value of N/h. We find

$$H = -\frac{\alpha \ln \alpha}{1-\alpha} N. \qquad (5)$$

The expected number of revisions, S say, in a successful set is then

$$S = \sum_{n=1}^{\infty} n\,(1-\beta_n) \prod_{k=1}^{n-1} \beta_k, \qquad (6)$$

provided that the series converges.

The expected total number of revisions, E say, to achieve a valid conclusion, is given by

$$E = HS. \qquad (7)$$

We now examine the consequences of the alternative assumptions previously made regarding the β_n's.

§2.1 $\beta_n = \text{constant} = \beta'$. From (5), (6), (7) we find

$$S = \sum_{1}^{\infty} n(1-\beta')\,\beta'^{\,n-1} = 1/(1-\beta'), \qquad (8)$$

and

$$E = -(\ln \alpha)\frac{N\alpha}{(1-\alpha)(1-\beta)}. \qquad (9)$$

§2.2 $0 < \alpha < \beta_1 < \beta_2 < \ldots < \beta_n < \beta_{n+1} < \ldots < 1.$

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[1] W. Feller, *An Introduction to Probability Theory and its Applications*, New York, 1950, 1, Ch., 11, §3

The convergence of the series (6) is investigated by Raabe's test, a sufficient condition being

$$\beta_\infty = \lim_{n\to\infty} \beta_n \to K < 1. \tag{10}$$

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Entropy and Disorder

With much interest I read the note by Dr Martin J. Klein entitled 'Order, Organisation and Entropy'.[1]

Dr Klein's argument is essentially correct, but the thesis brought forward obtains its proper significance only when the sentence 'entropy being the physicist's measure of disorder' (p. 158) is omitted. That sentence indeed is not true. Entropy is a measure for the number of microscopically distinguishable states which are compatible with a state of a system, specified in the usual macroscopic way (the calculation of this number is the subject of statistical quantum mechanics). This conception is already contained in Dr Klein's words 'the entropy is low when the number of energy states, in which there is a non-negligible probability of finding the system, is small' (p. 159). When the argument is considered from this angle, it is not at all strange that under comparable conditions a more highly organised system may have a greater number of distinguishable states than a less organised system and thus have a greater entropy.

I believe that the use of the word 'disorder' in connexion with entropy is a mistake. It leads to anthropomorphic forms of reasoning which are often misleading. There is no 'disorder' in nature; any arrangement of the parts or of the states of a system has the same value from an objective point of view; it is human preference to consider an arrangement ordered, e.g. according to the sequence of natural numbers, as something different from any other arrangement. The assertion 'a system left to itself tends to a greater disorder' has no real sense; on the other hand the assertion that, in certain cases (with the exception of states to be reached only via ‘ forbidden ’ transitions), ‘ a system realises all distinguishable states compatible with its macroscopic specification ’ conveys a definite meaning and gives a basis for quantitative evaluation.

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[1] M. J. Klein, 'Order, Organisation and Entropy', this *Journal*, 1953, **4**, 158–60.

With this assertion the conception of entropy as a measure of ‘ order ’ or ‘ disorder ’ can entirely be discarded and there is much less chance for being led into fallacious speculations.

The further advantage is gained that it becomes conceivable that for a being which could make more refined observations than is possible with our present physical methods, the entropy of a system would be reduced, since the number of microscopically distinguishable states compatible with the specification given by such a being would be less. In the ultimate case of a hypothetical being sensitive to complete microscopic specifications, every specified state of the system would be unique and would have zero entropy.

Human beings seem to be able to act on certain distinctions which cannot be explained as a result of mere physical or chemical reactions, as far as we understand them at present. If the hypothesis is ventured that this is the outcome of some general property of all living beings, the proper formulation of which still is beyond our knowledge, new light might perhaps be shed on the meaning of entropy relations in life.

Postscript: Dr Klein, in a letter directed to the undersigned, has asked me to mention that he intended the word ‘ order ’ to have no other quantitative significance than is given by its definition in terms of entropy, and that there is no real difference in basic point of view between us. He adds that the use of ‘ disorder ’ as a synonym for ‘ entropy ’ did not originate with him.

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REVIEWS

INSTINCT

THE concept of instinct has passed through many adventures and vicissitudes during the course of its long history. The central idea which it denotes —namely, that of behaviour driven from within—goes back to Thales (640–546 B.C.). But of course a word which has had such a long history and has become such an important item in our everyday vocabulary has inevitably acquired many variations, overtones and complexities of meaning which have been the despair of biologists. It is therefore not surprising that as soon as the work of zoologists and physiologists had provided what appeared to be a fairly adequate skeleton of a reflex theory of animal behaviour, i.e. in the early decades of the twentieth century, the concept of instinct was increasingly avoided and soon became almost disreputable. In fact the physiologist, experimental zoologist and comparative psychologist had almost completely abandoned the word by 1920 and it lingered only in the vocabulary of the psychologist concerned with human behaviour and in that of the more naturalistic type of zoologist.

This situation, as we see it now, was obviously a false one, for the rejection of the term meant that an essential idea was being left out of biology and psychology—although in fact it was already slipping in through the back door under the alias of ' drive '. So it was merely a matter of time before its loss began to be felt and the way opened for its return, although the rapidity and completeness of that return has surprised even its strongest supporters. The publication of a full and authoritative book [1] on the subject by one of the foremost of modern experimental field naturalists is therefore a noteworthy event for all those interested in biological theory—although it is unfortunate that the publishers should have found it necessary to delay so long the issue of a work dealing with a branch of biology which is just now moving so rapidly.

Dr Tinbergen's book is an admirably clear, authoritative and factual account of recent work on animal instinct which can be strongly recommended to students of biology, both elementary and advanced, as giving a first hand account of the main trends of modern field research into animal behaviour. It is also particularly valuable in skilfully summarising a great deal of European work mainly published in German and some of it not too easily accessible to English readers ; though it may be criticised as doing somewhat less than justice to the very active American workers in comparative psychology. Such American schools as those of Hull and Tolman, although (because of their concentration on the white rat and its maze running abilities) in some ways out of touch with some of the most interesting biological problems of the day, have nevertheless provided a vast amount of exact information on the learning process. It is true that much of their work seems dull and hard to relate to the problems of modern zoology, primarily because of their failure to follow the lead given in their own country by Wallace Craig in distinguishing between appetitive behaviour and consummatory act as constituents of instincts. Nevertheless many facts are there and it would have been a help to all types of reader if they could have been brought into proper relation with the studies of those who call themselves ' ethologists '.

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[1] *The Study of Instinct,* N. Tinbergen (Clarendon Press, Oxford, 1951, pp. xli + 228, 25s.)

Konrad Lorenz may truly be said to be the founder of the modern school of ' ethology ' or comparative behaviour study. For although many of its essential ideas were the product of men like Heinroth, Lashley Whitman and the above mentioned Wallace Craig, yet it was Lorenz who —arriving at many of them independently—welded them into a practically useful system and, by his concepts of appetitive behaviour, consummatory act, action specific potential, releaser, vacuum activity, etc., produced for the first time a general scheme sufficiently precise and comprehensive to make the problem of instinct seem attractive to the research worker. Previously all was darkness, mystery and confusion and one could hardly see where to start experimenting. But after Lorenz's work, although there was mystery in plenty still about, it was confined and regulated so that there were many parts of ' instinctive behaviour ' where the mystery seemed enough to be stimulative and provocative without being so great as to be daunting and overwhelming.

The concept of nervous hierarchy is a particularly important feature of Tinbergen's development of Lorenz's system. The idea was first introduced into human psychology by Herbert Spencer but Tinbergen has resuscitated and elaborated it in a most fruitful manner. He thus produces a scheme for the symbolic representation of instinctive behaviour involving a series of ' centres ' (which are regarded as ' functionally characterised systems not necessarily with any precisely limited anatomical location ') organised in a hierarchy of different levels, each level being activated by a drive descending to it from the one above. This scheme has now become well known and is proving extremely valuable in stimulating and co-ordinating experiments on instinctive behaviour. It suggests, although it does not necessarily demand, that there must be somewhere in the nervous system a structural organisation. This need not necessarily be physically localised in centres which will ultimately be revealed by histological methods but may be fixed and co-ordinated in some way not yet understood. For the loading and release of the hierarchies of this hypothetical neuro-physiological mechanism, internal and external factors are necessary, and the organism responds to factors by co-ordinated movements each apparently directed towards the attainment of a specific goal. Thus, the highest centre of all is supposed to provide motivational impulses which flow down without hindrance into the next centre below, which centres are regarded as being at the level of a major instinct—say reproduction. Besides these intrinsic motivational impulses, whose existence is still unproved though they seem probable enough, other factors impinge upon the centres of the major instinct. These are (1) Hormones, or other chemical stimuli ; (2) Sensory stimuli coming from the viscera of the animal ; (3) Impulses which may be intrinsic to the centre itself ; and (4) External stimuli loading the centre from outside by means of the external sense organs. In the particular instance on which Tinbergen has been specially concerned, namely the stickleback, this centre at the level of the major instinct would be a territorial centre, that is, the centre which activates the behaviour concerned with taking up a territory in spring.

In each of the channels which lead downward from this centre there is supposed to exist a physiological mechanism which effectively prevents or blocks all discharge of activity unless the animal encounters the right environmental situation or stimulus to remove or release the block. Thus there are incorporated in each level of the instinctive organisation a number of Innate Releasing Mechanisms (I.R.M.), as was postulated by Lorenz, which are in some way attuned to the biologically right stimuli or situations in the environment—as, for instance, the appearance of the sexual partner or the specific prey—and which are, as it were, unlocked by the appropriate releasers, thus allowing behaviour to proceed to the levels below. These in their turn incorporate blocks and while these are unmoved, action of these lower centres cannot proceed. As long as they remain blocked the activity potential will therefore be diverted into the type of action known as Appetitive Behaviour, which is directive in that it tends to bring the animal into the kind of environment or situation where the appropriate releaser is likely to be encountered.

Perhaps one of the weaknesses of Tinbergen's picture of centres arranged in hierarchies and loaded with motivational impulses is the subtle way in which it seems to be leading back to the concept of ' drive ' as some sort of directive force or energy provided by the higher centres and coming down a series of channels to run into particular effectors. This is a concept which Lorenz's scheme, in which the motivation for a particular act is generated by the co-ordination mechanism of the act itself, seemed once to have disposed of quite satisfactorily. Now of course the directiveness can consist in the organisation of the nervous ' channels ' themselves and not in the drive. But this is only feasible as an explanatory system when the behaviour is sufficiently stereotyped to make reasonable the assumption that there are fixed nervous pathways for all varieties of actions observed. Once the actions are found to show more than a limited degree of variability then the explanation in terms of pre-formed pathways appears increasingly improbable. And indeed it was one of the weaknesses as well as the merits of Lorenz's formulation that behaviour which is really difficult to account for was pushed aside into the ' appetitive behaviour ' category and was apt to be left there unexplained. Now much appetitive behaviour does undoubtedly show signs of what can only be described as purposiveness and it seems that much of it must be self-rewarding and self-stimulating until the full releaser for the next stage of the appetitive behaviour has been achieved. Thus it often appears as if the releasers which are active during the progress of the appetitive behaviour act not merely as releasers of Specific Action Potential which has already accumulated, but also as a stimulant enabling the behaviour concerned to lay hold of, or utilise more action potential. In other words, much appetitive behaviour is, in a sense, also consummatory act.

It is of course primarily in oriented, as distinct from unoriented, appetitive behaviour that the system can be self-stimulating and re-exciting ; and in such a process learning plays an essential part. Psychologically it appears as if the animal is both self-re-exciting and is also responding to dimly-seen goals which are striven for—though of course at extremely short range only.

It seems, then, that to account adequately for all varieties of appetitive behaviour it is necessary to assume that the fixed action-patterns, the performance of which constitute the consummatory acts of the various instincts, are—by feed-back mechanisms or some other devices—effectively generating some specific action potential, and that some extremely short-term ' purpose ' in the drive itself must also be postulated. The concept of appetitive behaviour as the expression of nothing more than a completely non-directive force, or drive, rigidly confined within a previously co-ordinated system of conduits, must be inadequate.

Finally Tinbergen claims that the particular value of his system is that it is objective. This is undoubtedly true when it is compared with much of the naturalistic comparative psychology which was not infrequently met with prior to the publication of Lorenz's main papers, and its objectivity is certainly an inestimable advantage. But there is a serious danger lest objectivity come to be thought of as the sole requirement of a system of behaviour study. Actually, to the present writer, the great merit of the behaviour theory of Lorenz which Tinbergen has developed so notably and to which he has added so much of value is that it combines both objectivity and subjectivity and shows where each has its rightful place and each its dangers. Subjective and objective concepts are both necessary in the scientific study of life and a biology which succeeds in being purely objective (however successful it may be in parts of its field) must fail to provide a full biological philosophy—just as surely as a purely subjective biology must of course fail. In other words, subjective concepts derived from introspection are among the essential tools for the study of life and the only hope for a complete biology is to combine subjective and objective in the right proportions, using each approach, with due circumspection and adequate safeguards, as it is required. All our concepts, in whatever field, are of course in the last resort subjective in that they are the conscious expressions of our own experiences ; but we have above all to beware of falling into the trap—which has so frequently caused the downfall of past students of living matter—of supposing ourselves objective when in fact we are being subjective. First and all the time we must know exactly and precisely where and to what extent we are using and relying upon subjective concepts. Only so can biology hope to deal adequately with the vast theoretical problems that confront it. Above all must it avoid the sterile and futile division into opposing vitalist and mechanist camps which the experience of the past hundred years has shown to be philosophically so stultifying. D'Arcy Wentworth Thompson in the introduction to his great work *On Growth and Form* has much to say on this and kindred subjects which biologists, psychologists and philosophers would do well to read and re-read. We may quote one sentence here : ' Still, all the while, like warp and woof, mechanism and teleology are interwoven together, and we must not cleave to the one nor despise the other ; for their union is rooted in the very nature of totality.'

W. H. Thorpe.

Thinking and Experience, H. H. Price (Hutchinson's University Library, London, 1953. Pp. 365. 25s.)

According to the author, the most characteristic feature of thinking is recognition ; and the most characteristic feature of the world is that recognition is possible. Our basic experience consists in the fact that there are Natural Kinds of objects ; things recur and resemble each other ; and so we have either the Philosophy of Universals or that of Ultimate Resemblances. Both philosophical theories, we are told, are acceptable and complement one another, though there are some reservations. For the one theory may mislead us into regarding universals as entities and also into taking the world as a more tidy place than it really is. But the other theory will remind us that the resemblances are by no means exact and so restores to thought and language the aura of haziness that by rights belong to them.

From the ontological presupposition about the nature of the world, the author turns to the epistemological assumptions about the nature of human intelligence. Without recognition there would be no cognition. And thinking is concerned with concepts, either basic or derived (i.e. ' directly abstracted from observed instances ' or acquired ' by intellectual operations performed upon these basic ones ', p. 38). Correspondingly, primary recognition can never be erroneous, though the secondary recognition by means of signs may be so. However, sign-cognition is indispensable since we need it to deal with what is not immediately experienced : it is ' an inductive mode of thinking ' (p. 88).

This gives rise to two kinds of meaning, that is, for signs and for symbols. Indeed, in the sentences e.g. ' black clouds mean rain ' and ' " rain " means rain ', the meaning of ' meaning ' is not the same. Nevertheless, we are advised to drop the distinction between natural and conventional signs. This advice is entailed by the decision that individual words, and not only sentences, are to possess an independent meaning (not derived from their use in a sentence). In this way, an Inductive Sign theory of language can be constructed, with the help of ostensive definitions. These are, of course, not definitions in the customary sense ; rather, they ' enable us to use (basic symbols) for ourselves as instruments to *think* with ' (p. 212). Ordinary definitions merely add words to our vocabulary ; but here we must build up our language ' *ab initio* ' (p. 214). An ostensive definition is, therefore, closely connected with induction ; and ' ostension ' or ' ostensive process ' would be a better name for it. Or, in yet another terminology, we may say that a symbol is linked to the object by psychological association ; and so the ostension becomes a *rule* which possesses a *sanction*.

It follows then, that thinking—at least sometimes—must be an operation with mental images, though this is an unfashionable view today. ' . . . It would appear that some very clever people do their thinking . . . almost exclusively in words. But it is a misfortune for a philosopher to be too clever. It is the human mind in general, and not just the minds of highly intellectual persons like himself, which he is supposed to be studying ' (p. 235). Anyway, the contemporary philosophers have overlooked the ambiguity of the verb ' to imagine '. ' It does sometimes mean " imaging " ', and this ' is the process-word which we must use ' (p. 247). Images, however, are not imaginings and, in a wider sense (which includes, e.g., metaphors) they do function as ' general symbols ' (p. 265). This leads to the classical theory that ' thinking is differentiated from other forms of cognition . . . by having a special sort of *objects*, which are variously called universals, concepts, or abstract ideas ' (p. 301). This theory is accepted here as substantially correct though, perhaps, it is a little too simple ; for ' the word

" concept " is *often* used in a dispositional sense ' (p. 320). Though the dispositional properties of an object depend on its actual structure (e.g. being elastic is a function of the crystal structure), we need not always actualise our concepts. It is merely a question of memory ; for ' the basic empirical concepts are memory-dispositions ' (p. 358). There is nothing supernatural about memory : and in this way, so the author hopes, we may avoid the metaphysics of Platonic entities.

This brief summary does less than justice to the painstaking and balanced discussion which is to be found in this book. But one thing is clear : what we are offered here is traditional epistemology argued in the traditional manner. If we start with certain assumptions about the Nature of the World and of Man, and if we believe in a ' basic experience ', then everything else follows without fail ; and there is no need, nor indeed room, for scientific experiment. *How* we know leads to *that* we know and even to *what* we know.

But to assert generally that things resemble one another is not a statement of fact. We must say which particular thing resembles another particular thing in a particular way, e.g. an electron I observed today in a Millikan experiment has the same charge as the one I observed yesterday. At best the philosophic theory of resemblance is a proposal for speaking in a certain way ; and we could equally propose to say that all things are unique. Indeed, the author admits that, except as an ideal, there are no exact resemblances and speaks of ' degrees of resemblance '. But how do we determine these degrees ?

Nor is it true to say that ' we can all agree that thinking is . . . conceptual cognition ' (p. 313). What, pray, is cognition ? And is conceptual cognition *of* or *by means of* concepts ? And how do I cognise concepts ; or how do I cognise by means of, or through, or with, them (like seeing by means of spectacles) ? Nor can we accept that ' Intelligence is that special kind of cognition whose objects are universals ' (p. 56). Is that a basis for an I.Q. test (however bad these tests may be) ? Nor can we say that we think *with* symbols, as we think with our brain or run with our legs. Thinking *about* physics involves the use of symbols ; but does this make a symbol an instrument, like a hammer or a hacksaw ? Nor do we have to explain how language starts, e.g. from a *tabula rasa* even, in order to find the meaning of our words. Indeed, we can *give* meaning to the word ' squongle ' only after *taking over* the meaning of many other words : for how can we speak of ' meaning ' unless we accept, as a standard, some sentences, within a specific context, as being meaningful ? A causal theory of meaning can never help us unless we believe in an ' absolute ' beginning of language, that is, accept the Tower of Babel story.

As long as the philosopher remains within the ambit of the unscientific, or at best pre-scientific, theories inherited from the Greeks, there are no problems but only puzzles, and no solutions. All we can obtain is, according to the ontology assumed, one -ism or another. However attractively these -isms may be presented to us, *Quidquid id est timeo Danaos et dona ferentes.* If nowadays many philosophers reject this tradition, it is not (as the author slyly remarks) because they will not acknowledge that 'In philosophy, as elsewhere, it is perfectly possible that our ancestors were wiser than we are' (p. 304). But did they know as much as we do today? That is the question!

After all, two thousand years have since gone by; and we do not blame Aristotle for not knowing Galileo's experiments. Far from it. We admire our ancestors for having started it all, with so little evidence to go on; but we need not lapse, for that reason, into a Chinese ancestor-worship. Indeed, this would be unfair, and little respectful, to them. If we reject ontology and epistemology *in toto*, it is because our whole attitude has changed towards what constitutes a problem and how to solve it. For we had a lot of time in which to learn—little as it may be—by developing science and by recognising the mistakes which, quite naturally, our ancestors made when they first began.

Philosophy was once the pursuit of all knowledge; slowly, the various sciences split off from it. Today, no one would seriously argue about atomic theory in the way Leucippos and Democritos did; and philosophers no longer discuss the Nature of mass and force. Physics has become a science in its own right. At best, what philosophers can do is to discuss the usage of words like 'mass' and 'force'; it is the usage which scientists, and ordinary people, make on the basis of whatever information they possess; and to investigate this usage is an important task since it helps us to make clear what we mean by our words. Why should there be a special, i.e. philosophic, theory of thinking, independent of the science of psychology? Why should we retain the philosophic theories about Human Nature which were invented only because a scientific theory of human nature was, then, still lacking?

For this reason, also, the author's remarks about the 'Verbalists' do not really hit the so-called language philosophers. The gentle, though firm, manner in which he deals with his opponents is charmingly different from the acrimony so frequent in philosophic debate; yet, all the same, he does not succeed. One may sympathise with his complaint that we 'live in the most word-ridden civilisation in history' (p. 252). However, logorrhea has always been an occupational hazard to philosophers—in fact, the traditionalist is much more prone to it than the modernist. It is one reason why contemporary philosophers have become so linguistically-minded.

Within the narrow limits which the author has set himself this book is an admirably lucid account of the traditional theory of knowledge. One can see clearly its basic circularity: arguing from what there *is* to what we *know* (*via* thinking) and back from what we *know* to what there *is*. But no closed theory—in contrast to the open theories of science—can ever be more than an attempt, however ingenious, of *τὸ ἥττω λόγον κρεῖττω ποιεῖν*.

E. H. Hutten.

Many-valued Logics, J. B. Rosser and A. R. Turquette (North-Holland Publishing Company, Amsterdam, 1952. Pp. viii + 124. 19 guilders.)

Ever since the first clear enunciation of the principle 'Every proposition is either true or false' there have been those who questioned it. This book contains a thorough treatment of many-valued statement and first order predicate calculi. It is well got up, fully and clearly written. No previous knowledge of many-valued logics is required but good acquaintance with ordinary two-valued first order predicate calculus is. The results, practically entirely due to the authors, consist in constructing and formalising very general forms of many-valued logics. No meaning or interpretation is given, the authors rightly considering that precise formal development must first be carried to an even higher level of perfection.

The development is general. Suppose given a list of symbols and a certain non-empty subset of formulae (finite linear succession of symbols) called statements, denoted by P, Q, . . . Also given b functions of statements $F_i(P_1, \ldots, P_{a_i})$, $1 \leqslant i \leqslant b$, which are also statements. Also given M integers 1, 2, . . ., M called truth-values of which 1, . . ., S, $S < M$, are called designated. Also given a truth-function $f_i(p_1, \ldots, p_{a_i})$ associated with $F_i(P_1, \ldots P_{a_i})$, with arguments and values from 1, 2, . . ., M. Thus given $F_1(P_1, P_2)$, $F_2(P_1)$ with associated truth-functions $\max(1, p_2 - p_1 + 1)$, $M - p_1 + 1$, then, by substitution, functions of statements $J_k(P)$ with associated truth-functions

$$j_k(p) = \begin{cases} 1, & p = k, \\ M, & p \neq k \end{cases}$$

are definable. So are

$$P \vee Q \text{ as } F_1(F_1(P, Q), Q),\ P \,\&\, Q \text{ as } F_2(F_2(P) \vee F_2(Q)),$$

$\overline{P}$ as $J_{s+1}(P) \vee \ldots \vee J_m(P)$, $P \supset Q$ as $\overline{P} \vee Q$ with associated truth-functions $\min(p, q)$, $\max(p, q)$,

$$-(p) = \begin{cases} 1, & p \text{ undesignated} \\ M, & p \text{ designated} \end{cases} \qquad \supset(p, q) = \begin{cases} 1, & p \text{ undesignated} \\ q, & p \text{ designated.} \end{cases}$$

In terms of $\max(1, p_2 - p_1 + 1,) M - p_1 + 1$ we cannot define all truth-functions, but add Słupecki's 'tertium function' $F_3(P)$ with associated truth-function $f_3(p) \equiv 2$ then functional completeness results.

If in terms of our basic functions we can define function of statements $\&(P, Q)$, $\vee(P, Q)$, $\supset(P, Q)$, $\sim(P)$, $J_k(P)$ for $1 \leqslant k \leqslant M$, with associated truth-functions satisfying :

(i) $\&(p, q)$ designated if, and only if, p, q both designated,
(ii) $\vee(p, q)$ undesignated if, and only if, p, q both undesignated,
(iii) $\supset(p, q)$ undesignated if, and only if, p designated, q undesignated,
(iv) $\sim(p)$ designated if, and only if, p undesignated,
(v) $j_k(p)$ designated if, and only if, $p = k$,

then we say that these truth-functions satisfy standard conditions. $J_1(P), \ldots, J_s(P)$ act like identity functions, $J_{s+1}(P), \ldots, J_m(P)$ like negation functions. A statement is to be asserted when, and only when, its corresponding truth-function takes designated values exclusively.

There is an alternative procedure for accepting statements which makes no reference to their truth-values. This is the axiomatic method, we choose certain statements as acceptable and certain rules for generating other acceptable statements from given acceptable statements. If every statement acceptable by a truth-value stipulation is also acceptable by an axiomatic stipulation we say the axiomatic stipulation is deductively complete. If every statement acceptable by an axiomatic stipulation is acceptable by a truth-value stipulation we say the axiomatic stipulation is plausible. If both hold we say the two stipulations are equivalent, then both define the same set of acceptable statements. There are given seven axiom schemes and one rule (modus ponens). It is supposed that functions of statements $P \supset Q$, $J_k(P)$, $1 \leqslant k \leqslant M$ can be defined in terms of the basic functions. A 1-3 involve $\supset$ only, A 4-6 $\supset$ and J_k, while A_{-} involves the F_i as well, e.g. $J_i(P) \supset J_{m-i+1}(F_2(P))$ as suggested by the truth-value stipulation. The axiomatic stipulation is then shown to be deductively complete and if $\supset(p, q)$, $j_k(p)$ satisfy standard conditions also to be plausible, while if the set of basic functions is functionally complete then there are $\supset$ and J_k such that the two stipulations are equivalent.

The treatment of quantification is general, first a truth-table stipulation then an axiomatic stipulation. Individual variables $X, Y, Z, \ldots$ and predicates $F(X)$, $G(Y_1, \ldots, Y_u)$ are added as constituents of statement formulae. A novel feature is to allow a quantifier which permits the binding of several variables at once, and the combining of more than one predicate. Quantifiers are introduced by c basic functions $\Pi_i(X_1, \ldots, X_{\beta_i}, P_1, \ldots, P_{\gamma_i})$, $1 \leqslant i \leqslant c$, $1 \leqslant \beta_i$, $1 \leqslant \gamma_i$ which are statements, the occurrences of $X_1, \ldots, X_{\beta_i}$ are bound, e.g. in the two-valued case $\Pi_1(X, P)$ might be $(X)P$. An example is $\Pi_1(X, Y, P, Q)$ determined thus :

(i) $\Pi_1(X, Y, P, Q)$ takes the truth-value 1 if, and only if, there is an X such that for all Y $F_1(P, Q)$ takes the truth-value 2 ;

(ii) . . ., (M) give conditions under which the remaining truth-values are taken.

Formalised versions of intuitive descriptions, such as the above, play an important rôle. Alternatively one might formalise the following intuitive description : $(X)P$ takes the truth-value R if, and only if, for at least one value of X from the universe of individuals P takes the truth-value R while for all other values of X, P never takes truth-values greater than R.

To formalise these concepts the method of 'partial normal forms' is used. Each statement is to have M partial normal forms. Roughly the Rth ($1 \leqslant R \leqslant M$) partial normal form $N_r(P)$ is a specification in terms of two-valued logic of the conditions under which P takes the truth-value R. For example, the first partial normal form of $\Pi_1(X, Y, P, Q)$ (case $M = 4$) is

$$\sim (x) \sim (y)\{(q_2 \,\&\, p_1) \vee (q_3 \,\&\, p_2) \vee (q_4 \,\&\, p_3)\},$$

where p_k denotes 'p takes the value k', etc., for q. Then $N_r(P)$ is translated into a statement $n_r(P)$ in the many-valued logic. The partial normal forms must be mutually exclusive and exhaustive, for no statement is to have two different truth-values under the same circumstances and always must have at least one. By means of the normal forms a truth-value stipulation for acceptable statements is defined. Finally, standard conditions for a many-valued operator (X), analogous to the two-valued universal quantifier, are given, roughly that $(X)P$ takes a designated truth-value if, and only if, P always takes a designated truth-value.

Three axioms of quantification are given. The first two involve $(X)P$, supposed definable in terms of the c basic functions, and are similar to the two-valued case. The third

$$A10 \,.\, n_r(\Pi_i(X_1, \ldots, X_{\beta_i}, P_1, \ldots, P_{\gamma_i}) \supset J_r(\Pi_i(X_1, \ldots, X_{\beta_i}, P_1, \ldots, P_{\gamma_i}))$$

is suggested by the truth-value stipulation.

It is then shown that if the functions $\supset (p, q)$, $j_k(p)$, $\sim (p)$, $N_r((X)P)$ satisfy standard conditions the axiomatic stipulation is equivalent to the truth-value stipulation. The proof of this is a generalisation of Henkin's proof of the deductive completeness of the two-valued predicate calculus. Whence a many-valued generalisation of the two-valued Lowenheim-Skolem theorem that a satisfiable set of many-valued statements is satisfiable in a denumerable domain of individuals.

Then follow illustrations :

(i) Łukasiewicz-Tarski, $F_1[= C]$, $F_2[= N]$, $\Pi_i(X, P(X))[= (X)P(X)]$; $S = 1$;

(ii) same except $S > 1$;

(iii) same as (i) but with addition of $F_3[= T$, Słupecki$]$, $S \geqslant 1$. This is functionally complete, (i), (ii) not so.

The book ends with a few remarks on unsolved problems, one of which is possible useful applications. The requirements of a many-valued calculus are clearly stated in thirteen hypotheses, e.g. the existence of b basic functions $F_i(P_1, \ldots, P_{a_i})$.

S. W. P. STEEN

An Essay on Method, C. Hillis Kaiser (Rutgers University Press, New Brunswick, N.J., 1952. Pp. vi + 163. $3.25)

DR KAISER sets out to give 'a comparative analysis of the methods of art, science, philosophy and religion in order to determine the kinds of problems which can be solved by the methods of each', to construct, in the manner of R. G. Collingwood, a 'map' showing the relations between these four fundamental disciplines. His admitted debt to Collingwood does not, however, entirely determine his conclusions.

The book is short, perhaps too short to do justice to its theme, so that it is not always made clear what precisely is involved in the relations asserted to hold between the disciplines. Some of the conclusions need more detailed discussion, and there is often a lack of support for what appear to be wise and considered remarks.

Art and science, clearly distinguished from craft and technology, are classified as 'playful' rather than 'serious' activities since their outcome does not have an immediate bearing on the well-being of those individuals affected by them. Philosophy and religion, on the other hand, are serious activities. 'Religion, in fact, seems to be the prime example of a serious activity . . . ; it is born of a passionate desire to achieve the well-being of oneself or others . . .' and philosophy, Dr Kaiser holds in the teeth of all opposition, is 'concerned directly or indirectly with human happiness and human destiny', with God, freedom and immortality.

Science and philosophy involve judgment since they seek to state 'what is the case', religion and art do not. Religion seeks to exhort or command and 'aesthetic judgments' are merely reactions of liking, disliking or preference. Thus only philosophy and science make statements, the distinction between them being that scientific statements are verified by 'logical inspection' or 'direct apprehension in sensation or imagination' while philosophic statements are not verified in these ways for they refer to entities and relations not accessible to immediate experience. Art shares the immediacy of science, religion the 'transcendental' character of philosophy. The religious *a priori*, however, is 'a disposition of the self toward moral perfection' whereas the philosophic *a priori* is 'a transcendental ground for the knowledge at which philosophy aims'. Philosophy and art are entirely unrelated, as are science and religion. Thus the disciplines can be arranged in a sort of ' square of opposition ' at the corners of which are, in order, philosophy, religion, art and science but with no diagonals joining philosophy with art and science with religion.

Most of us can probably find something to dispute in this account. The distinctions are perhaps too sharply drawn ; some connections have perhaps been omitted. But such criticisms are less important than a consideration of Dr Kaiser's general approach and I shall content myself with a brief comment on this.

The delineation of the ' four fundamental disciplines ' does not seem to conflict in Dr Kaiser's mind with his assertion that methodology is ' an independent discipline which is not identical with any of the fundamental disciplines '. The method is comparative and its business is to describe how people behave when they are functioning as scientists, artists, etc., for one knows what a discipline is only when one knows how it is done. He insists that he is doing ' objective methodology ' which seeks to establish the methods actually used, rather than ' polemic methodology ' which seeks to persuade us that one method is better than another.

If we accept the view that to discover what science, for instance, is we must discover ' how scientists behave when they are functioning as scientists ' there is an obvious danger of circularity as long as we regard methodology as an empirical study. Dr Kaiser seeks to avoid this circularity by defining, at the outset, the words used to denote the disciplines with reference to ' the core of agreement ' in ordinary usage and then showing how the behaviour of practitioners in the four fields fits his definitions. The objection to this method is that deciding what is the core of agreement appears to involve selection, on the basis of *a priori* principles, from among different ordinary usages and the illustration and ' verification ' of the definitions to involve selection and interpretation of procedures, again on the basis of *a priori* principles. This seems clear from Dr Kaiser's conclusions for his final accounts of science and philosophy, at least, appear often to conflict with ordinary usage. This, of course, is not in itself a criticism, but only in so far as it suggests that he is not always doing what he claims to do.

A different approach would surely have been more illuminating. Certain ' results ' (I use this vague word in the hope that it will cover all the disciplines) purport to be, or are generally accepted as, scientific, philosophical, artistic and religious. If we accept these provisionally we can subject them to logical examination to discover how they can be arrived at or supported. Comparison with the ways in which results are in fact arrived at and supported will then bring out differences within and between disciplines. Even if it is true that we do not know what science is until we know how it is done, even if we insist that it is an activity, we can take the nature of actual conclusions purporting to be scientific as a rough guide to the nature of the activity. This procedure would be less likely to result in a map distorted by our preconceptions about the activity, more likely to avoid polemical methodology.

PETER ALEXANDER

Medieval Logic: An Outline of the Development from 1250 to c. 1400, Philotheus Boehner O.F.M. (Manchester University Press, 1952. Pp. xvii + 130. 12s 6d.)

WHEN Prantl in 1870 published the fourth and last volume of his *Geschichte der Logik* he said in his preface that he had often thought of some words of Lessing: 'No effort is wasted which may save the effort of another; my reading of the useless has not itself been useless if from now on this or that man can avoid the reading of it.' With the 'worst rubbish of logical literature' (i.e. presumably the works of the Middle Ages) behind him, he hoped there would be no need for another to repeat his enterprise, at least in the near future. This pronouncement deserves to rank among the Great Unfortunate Remarks. For the modern development of logic, which began in Prantl's own time with the work of Boole, has now made it clear that his standards of judgment were woefully inadequate. To us some of the most interesting passages of his book are his quotations in footnotes of Stoic or medieval novelties that he has ridiculed in his text. But the reassessment of medieval logic is an enormous task. The bulk of the writings which survive is so great that it can scarcely be expected they will all be published in modern editions, and no doubt many of them are, if not rubbish, then second-rate stuff. It is necessary, however, that they should all be re-read and made the subjects of reports by persons who are at once good medieval scholars and sufficiently familiar with modern logic to recognise an interesting novelty when they see it. Only then will it be possible to produce a satisfactory history of the intellectual life of the period. In a recent publication of the Institute of Franciscan Studies Father Boehner surveyed a number of medieval tracts *de futuris contingentibus* in order to determine whether or not any notion of a three-valued logic could be found there. In the present work he has taken a more general view, but he explains in his preface that he can present only samples from a relatively unexplored field with the hope of conveying an idea of genuine scholastic logic to modern logicians and to those philosophers who call themselves neo-scholastics. The first group of readers will certainly be interested, and it is to be hoped that the second will be also. For if some more of the enthusiasm for medieval philosophy which has been aroused among members of the Roman Catholic Church during the present century could be directed into studies of the kind indicated above, the deprantling of the history of logic might be hastened on.

In his first part Father Boehner enumerates the items of the legacy which medieval logicians inherited from antiquity, following the titles of the logical tracts attributed to Albertus Magnus, and then explains briefly the new elements. In his second part he treats at greater length of three among the novelties which seem to him most important, namely, the theory of *syncategoremata* as logical constants, the theory of *suppositio* (or standing-for), and the theory of *consequentiae* (or implications). Finally, in the third part he gives short accounts of the logical systems of five prominent scholastics, Peter of Spain, William Ockham, John Buridan, Walter Burleigh, and Albert of Saxony. The fact that he does not deal with *insolubilia* (i.e. versions of the Liar paradox) at any length is due presumably to the lack of any detailed modern review of this subject. What Paul of Venice tells us in the fifteenth century about the doctrines of *cassatio* and *restrictio* worked out by his predecessors is enough, however, to show that some medieval logicians were very ‘ warm ’ in their search for a solution of the puzzle.

The theory of *suppositio* should interest modern logicians who write about the referring use of words, and the theory of *consequentiae* should interest all who know of the debates which have taken place in this century about material and strict implication. In connection with *consequentiae* it is rather surprising that Father Boehner devotes so much space to Ockham. For though Ockham was undoubtedly a very influential logician, his treatment of this topic is less clear and much less interesting than that in the *Exactissimae Quaestiones* once attributed to Duns Scotus. In the latter the paradoxes of both material and strict implication are presented very carefully with proofs, whereas in Ockham's *Summa Totius Logicae* they get no more than an unsympathetic mention. My own impression is that the discovery of the paradoxes produced a debate on implication in which later logicians tried to evade trouble by new definitions. Thus Ralph Strode, an Oxford logician of the generation after Ockham, did not define a *consequentia formalis*, as the author of the *Exactissimae Quaestiones* had done, by saying that it held in virtue of the *syncategoremata* as opposed to the terms, but rather by saying that it held *de formali intellectu* of all the expressions involved. And Strode's *Consequentiae* was still being read in Italy as late as the beginning of the sixteenth century. On the other hand, Father Boehner is rightly impressed by the efforts of some later logicians to make the doctrine of *consequentiae* basic in their presentation of logic, instead of a mere appendix, and he promises to produce an edition of Walter Burleigh's work, *De Puritate Artis Logicae*, in which apparently this programme has been carried out.

Modern interest in medieval logic is due largely to Professor Łukasiewicz, who drew attention also to the importance of Megarian and Stoic work, and it is natural to ask whether there is any connection between the discoveries of the thirteenth and fourteenth centuries A.D. and those of the fourth and third centuries B.C. Father Boehner seems inclined to think that medieval logicians got nothing from antiquity except the Aristotelian tradition. I cannot agree with this view. Admittedly the logicians of the medieval period do not talk about Diodorus, Philo, and Chrysippus, but some at least knew their Boethius well, and fragments of the Megarian-Stoic tradition are preserved in his works. In his *De Syllogismo Hypothetico*, for example, there is a discussion of different kinds of *consequentia*, i.e. of different requirements for the truth of a conditional statement. Another possible source is the *De Nuptiis Philologiae etMercurii* of Matrianus Capella. This is known to have been used by the wandering Irish scholars who helped to restore learning after the Dark Ages, and poor as it is, it contains a book on dialectic with a garbled version of the ' indemonstrable ' patterns of inference set forth by Chrysippus.

In general Father Boehner's writing is clear, but it sometimes bears marks of translation, and he uses the royal ' we ' in a way which is irritating to an English reader. The sign of existential quantification is printed wrongly throughout the body of the text, though it is given correctly in the initial explanation of symbols.

William Kneale

The Works of George Berkeley, Bishop of Cloyne, edited by A. A. Luce and T. E. Jessop, Vol. VI, edited by T. E. Jessop (Thomas Nelson & Sons Ltd., Edinburgh and London, 1953. Pp. viii + 255. 30s.)

This volume contains Berkeley's contributions to ethics and other miscellaneous writings, which fall outside the field of this *Journal*. But readers who are interested in Mr T. W. Hutchinson's paper on Berkeley's economics in the bicentenary number will find the texts here—mainly *The Querist* with one or two addenda, but also *An Essay towards Preventing the Ruin of Great Britain.*

J. O. Wisdom

RECENT PUBLICATIONS ON THE PHILOSOPHY OF SCIENCE

(*a*) Books Received for Review

Inclusion of books in this list does not preclude their being reviewed in later issues

Agnes Arber, *The Mind and the Eye*, Cambridge University Press, 1954, pp. xi + 145, 16s.

Ernst Cassirer, *The Platonic Renaissance in England*, translated by James P. Pettegrove, Thomas Nelson & Sons Ltd., Edinburgh, 1954, pp. vii + 207, 15s.

Morris Raphael Cohen, *Reason and Nature: an Essay on the meaning of Scientific Method*, The Free Press, Glencoe, Ill., 1953, Revised Edition, pp. xxiv + 470, $6.00.

C. M. Focken, *Dimensional Methods and their Applications*, Edward Arnold & Co., London, 1953, pp. viii + 224, 30s.

Galileo Galilei, *Dialogue concerning the two Chief World Systems*, translated by Stillman Drake, foreword by Albert Einstein, Cambridge University Press, 1954, pp. xx + 496, 75s.

Augusto Guzzo, *La Philosophie de Demain*, Aubier, Editions Montaigne, Paris, 1953, pp. 105.

'Homage to George Berkeley', *Hermathena*, published for Trinity College, Dublin, by Hodges, Figgis & Co., Dublin 1953, pp. viii + 146, 10s.

G. E. Hutchinson, *The Itinerant Ivory Tower*, Oxford University Press, London, 1954, pp. xi + 261, 32s.

W. M. Ivins Jr., *Prints and Visual Communication*, Routledge & Kegan Paul, London, 1953, pp. xxv + 190, £2.2s.

F. R. Landi, *Charles Morris*, Fratelli Bocca Editori, Roma, 1953, pp. 295, L. 1500.

Hans Reichenbach, *Nomological Statements and Admissable Operations*, North-Holland Publishing Company, Amsterdam, 1954, pp. 140, £1.7s.

Otto Samuel, *A Foundation of Ontology*, Philosophical Library, New York, 1954, pp. xv + 155, $3.75.

Erwin Schrödinger, *Nature and the Greeks*, Cambridge University Press, 1954, pp. 97, 10s. 6d.

Alfred Tarski, in collaboration with Andrzej Mostowski and Raphael M. Robinson, *Undecidable Theories*, North-Holland Publishing Company, Amsterdam, 1953, pp. xi + 98, 18s.

Arnold Tustin, *The Mechanism of Economic Systems*, William Heinemann, London, 1954, pp. vii + 161, 25s.

G. J. Warnock, *Berkeley*, Penguin Books, Melbourne, London, and Baltimore, 1953, pp. 252, 2s.

(*b*) Journals

The contents of the latest issues of other journals are printed with the advertisements on the cover of this issue

Dialectica, Zürich (Switzerland), 1953, 7, No. 3:

R. McKeon, 'Process and Function'

A. Wittenberg, 'Über adaquate Problemstellung in der mathematischen Grundlagenforschung'

Methodos, Milano (Italy), 1953, **5**, No. 19.
  Ernest H. Hutten, ' The Mind-Body Problem '
  A. H. Basson & D. J. O'Connor, ' Language and Philosophy : Some Suggestions for an Empirical Approach '
  Edward G. Ballard, ' Operational Definitions and Theory of Measurement '
  P. W. Bridgman, ' Comments on the Paper of Edward G. Ballard '
  S. Ceccato & V. Somenzi, ' Operazionismo e tecnica operativa '
  Ricordi di Colloaui (S. Ceccato)
  David Londey, ' Geometry and Space '
Philosophy of Science Baltimore (Maryland), 1953, **20**, No. 4
  William E. Bittle, ' Language and Culture Areas '
  C. West Churchman, ' Concepts without Primitives '
  C. Wright Mills, ' Research in Current Social Studies '
  P. J. van Heerden, ' What is Matter ? '
  Cornelius L. Golightly, ' The James-Lange Theory : A Logical Post-mortem '
  Richard D. Chessick, ' Neurological Studies and Philosophical Problems '
  Herbert Hochberg, ' Empirical Philosophy of Roger and Francis Bacon '

(*c*) ARTICLES

Fr. Alverdes, ' Die Theorie der psycho-physischen Äquivalenz. (Das Leib-Seele-Problem) ', *Studium Generale*, 1954, **7**, 21

Ettore Carruccio, ' Significado Filosófico de la Lógica Matemática Contemporánea ', *Notas y Estudios de Filosofia*, 1953, **4**, 321

Silvio Ceccato, ' L'école opérationnelle et la rupture de la tradition cognitive ', *Bulletin de la Sociéte française de Philosophie*, 1952-3, **46-47**, 41

E. Schrödinger, ' Correspondence on What is Life ', *Humphreys History Behind Today's Events*, 1954, **7**, 48.

H. Förster, ' Allgemeine biologische Gesetzlichkeiten in Vergleich zu physikalischen Gesetzen ', *Studium Generale*, 1954, **7**, 40

Ernest H. Hutten, ' Natural and Scientific Language ', *Philosophy*, 1954, **29**, 27

J. v. Kempski, ' Handlung, Maxime und Situation. Zur logischen Analyse der mathematischen Wirtschaftsheorie ', *Studium Generale*, 1954, **7**, 60.

B. Klatt, ' Gedanken zur Zoologie als einer theoretischen Wissenschaft ', *Studium Generale*, 1954, **7**, 1

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